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ERRATUM.—In the last number of this Journal, page 576, 2nd column, line 11, the price of bromine is stated as 35s. per lb. Read *35 cents, or 1s. 6d. per lb.*

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MEETINGS, SESSION 1885—86.—First Monday in each month (unless otherwise indicated).

ORDINARY MEETINGS.

First Monday in the Month, at 8 p.m.

December 7.—Dr. P. F. Frankland on "Some Aspects of Filtration and other Methods of Water Treatment." Dr. F. L. Teed—"Note on the action of Calcium Carbonate on Solution of Ammonium Chloride."

January 1, 1886.—Mr. G. E. Davis, F.I.C.—"On the Enrichment of Coal Gas by certain Hydrocarbons." Mr. C. T. Kingzett—"Notes on Russian Turpentine."

February 1.—Mr. Gustav Bischof—"Notes on Dr. Koch's Water Test." Mr. J. W. Westmorland, A.R.S.M.—"The Estimation and Valuation of Copper Ores and Products for Commercial Purposes."

March 1.—Mr. Boverton Redwood, F.I.C.—"Viscosimetry." Mr. T. B. Lightfoot, M.I.C.E., M.I.M.E.—"Ice Making and Cooling Machinery."

April 5.—Professor Unwin, M.I.C.E.—"The Principles and Methods of Testing Cementing Materials."

May 3.—Messrs. Macnab and Beckett—"The Treatment of Water for Technical Purposes."

June 7.—Dr. Meymott Tidy—"Chemical Treatment of Sewage."

July.—Annual Meeting at Liverpool.

Notices of papers and communications for the meetings to be sent to the Local Secretary.

Meeting held Monday, October 26, 1885.

MR. D. HOWARD IN THE CHAIR.

HEATING REGENERATIVE GAS-FURNACES BY RADIATION FROM FLAME.

DISCUSSION.

THE CHAIRMAN commenced the proceedings by referring to the loss the Society had sustained by the death of the former Chairman of the London Section—Mr. Walter Weldon, F.R.S.—to whose memory and labours he paid a fitting tribute. Passing to the subject which was to occupy the attention of the members that evening, he said they were exceedingly fortunate in having amongst them the distinguished author of the paper which was heard with so much pleasure at the annual meeting of the Society in July; and he hoped the result of the discussion would be to throw some light on the practical application of the physical questions involved in the production of heat, which were of no little importance in these days of modern chemistry. He could not help thinking that they hardly appreciated how very much chemistry had to learn from the physicist—how much, in particular, with regard to the question of the full development of the force stored up in coal they had yet to learn. To say that in these days there was nothing more important than real economy in fuel was a mere truism; but, unless they could call in the aid of such men as Mr. Siemens, it was difficult to see how the resources which Nature had placed at their disposal could be thoroughly developed. They knew the theoretical power which ought to be obtained from 1lb. of coal, and they also knew (or perhaps a great many did not know) how much they fell short of this in practice in furnaces and boilers. There was before the physicist a vast field of improvement in the economy of coal, which might bring about a revolution quite as great as that produced by Watts's discovery of the steam-engine. He had great pleasure in asking Mr. Siemens to make any prefatory remarks he might feel inclined to offer; and he hoped all the members had taken an opportunity of carefully studying the paper, and would be fully prepared to discuss it, and show their appreciation of the importance of the problem before them.

Mr. F. SIEMENS: I may commence by explaining that, when I first altered the chambers of my furnaces so as to work by radiant heat, I had no scientific motives in view; I simply wished to improve the manner of working and the capabilities of my factory. I looked at the matter with the eye of a manufacturer who wanted to increase and cheapen production, and to improve the articles produced; to solve scientific problems was no part of my intention at that time. That is the reason why I worked this new system in my own establishments abroad for some time before publishing it. After having made considerable progress in the development of the invention at my Dresden glass works, heavy duties were imposed on glass entering Austria, and as I exported a large proportion of the products of my Dresden works to that country, I was compelled to construct new works in order to avoid paying the duty. I selected Neusattel, near Karlsbad, in Bohemia, as the locality for those works, and however regrettable it might be to me, from a manufacturer's point of view, to have to invest capital in their construction, they afforded me the opportunity of making further trials, as it is easier to design new furnaces to effect a special purpose than to alter old ones. These works came into operation in 1878, and although I obtained from the new furnaces improved results upon those obtained at Dresden, I did not yet see my way to take out patents, owing to the circumstance that the conditions involved in the new method of heating, as at that time practised, could only be explained from a scientific point of view, and that German patents are strictly limited to features of construction. Moreover, I desired to make further trials, and I felt sure of keeping the secret of the invention to myself, owing to the difficulty which would be experienced in discovering it from the outward appearance of a furnace. I was confirmed in this view by the consideration that the principles involved in my new furnace were directly opposed to the old notions of furnace construction and of combustion. But a few years later, after the gradual enlargement of my new works at Neusattel, and the construction of additional furnaces at Dresden and Doehlen, the experience obtained enabled me to mature my invention and to determine the features of construction essential to its success. This was about the time I took upon myself to carry on the work of my late brother, Sir William Siemens, and I determined to apply for patents and to publish the invention. The first memoir on the subject was the paper which I read before the Iron and Steel Institute, at Chester. On that occasion I kept strictly to the practical details of the furnace, leaving out theoretical questions altogether. I explained how, in order to bring about radiation, the flame was conducted through the furnace in such a way that it does not touch any part of the surface, either of the roof, sides, ports, or bottom, but is able to radiate heat freely in the chamber, and then go through the ports into the regenerators to deposit its remaining heat there by contact. In the first part of the operation the flame acts by radiation, in the second part by contact; this, as is shown by innumerable publications and descriptions of furnaces and systems, is the contrary of what has always been observed; there has always been a tendency to contract furnace chambers to the smallest possible size, and to bring the flame into contact with the side of the furnace and with the material to be heated; and where, on rare occasions, an exception has been made on account of the material which was to be operated upon not admitting, for some reason, of being touched by the flame, it was always explained why such a divergence from ordinary practice was necessary. During the discussion of my paper at Chester I described the theoretical part of the system, which has been taken up with great interest, most of the technical journals, and the writers for those journals, approving of it, some, however, having objected to it; and what surprised me was, that the principal objections were made on theoretical grounds, and ignored all the practical results, treating my work as mere scientific conjecture. On the whole, I am not at all dissatisfied with the opposition that has been raised against my new method of heating, and it was only what I could expect, as it interferes with many well-accepted arrangements and old-established views which will not be readily given up, although it can be shown that by supplanting them great practical results are obtained. Some have objected against the system that it is not new, because high roofs have been in use before. To this it may be answered that there is hardly anything new in form; the only question is with what intention a certain arrangement has been chosen. If, as has been the case, anyone says that he has seen furnaces with high roofs before, I can only ask him for what purpose such high roofs were adopted. There are various reasons which may induce a furnace builder to construct a high roof; besides, a high roof forms no part of my system, but it is only one of the means I employ to conduct the flame through the furnace-chamber in such a manner that only radiant heat comes into action there. If the high roof has not been selected for that purpose, and the flame continues to strike down on the bed of the furnace, such furnaces cannot be considered as constructed according to my system. I am still awaiting the production of any publication which shall show that furnace-chambers have ever been constructed in the same manner and for the same purpose as those described in my paper. Hitherto nothing has been brought forward to prove that my new method of constructing and working gas-furnaces has in principle been anticipated, even in the slightest degree. The scientific grounds on which objection has been made to my system were simply with regard to the laws of the dissociation of flame. It was maintained that it was utter nonsense to talk of accumulating heat in the way I described, because, when a certain temperature was arrived at, dissociation set in, and at such a high temperature no further combustion took place; so that however perfect the combustion might be, no advantage would be gained, because a certain temperature could not be exceeded. I could only set against this argument the fact that I was always able to increase the temperature, the only limit being the heat-resisting power of the material of which the furnace was built; so far I have not found any material sufficiently refractory to withstand the intense heat that may be obtained in the radiation furnace; but I think that further experiments I am making with that object in view will result in the production of a material more suitable for furnace building than any hitherto used. I have found that when furnaces, on the other hand, had small chambers, and the flame played directly on the sides, the inner walls, the arch, and the hearth, then certainly the heat did not rise to the same extent. This is a state of things which some call "dissociation;" but I attribute it to "imperfect combustion," brought about by the action of the surfaces. When the flame strikes in the immediate neighbourhood of the arch of a furnace, or of any solid material, the combustible gases cannot burn—they want elbow-room, as it were. After studying the question of dissociation, and the experiments which had been made, I came to the conclusion that the whole theory of dissociation of flame is not yet perfectly developed, or at least that no system of furnace construction can be fairly criticised from the dissociation point of view. I found that dissociation in an open space can only occur at a very intense temperature—so high, in fact, that we cannot reach it—and consequently it need not enter into any of our calculations of furnace building, always supposing that there is free space for the gas to burn in. On the other hand, most of the apparent cases of dissociation recorded admit, to my mind, of another explanation. It is the indirect influence of the inner surfaces of the apparatus employed which, in many cases, accounts for the appearance of dissociation. There are some experiments which prove this very clearly. I shall only refer to the experiments of eminent authorities, from which you will see that dissociation is always brought about by surface action. Bunsen established dissociation in the following manner:—He filled a tube with carbonic oxide and oxygen just in the right proportions to burn. He ignited it by some means, and produced an explosion; but he found that only one-third of the gases took part in this explosion, and the other two-thirds remained unburnt. He obtained, however, a temperature (according to his calculation) of 2400° C. Now, Bunsen relates that when he allowed a certain interval to elapse, so that the tube might cool down perhaps to 1000° C., he could produce with a portion of the remainder of the gas another explosion; the heat rose again, and the limit of explosion was again touched, so that no further combustion could take place. After cooling it a second time he produced a third explosion, and found that most of the combustible gases were consumed. Now, Bunsen says—and it seems very plausible—that, at a temperature above 2400° C., the gases do not enter into combustion, and that if you could raise the temperature higher by some artificial means, dissociation would set in, and the burning gases would separate. I would offer another explanation of these results, and this I am bound to do, to agree with the facts of combustion which I have proved to exist in regenerative furnaces worked by radiation. I am of opinion that only one-third of the gases filling the tube had sufficient space in which to burn; the other two-thirds were too near the circumference of the tube; they adhered to the sides, or struck on the sides, and consequently remained unburnt. When, however, a short time had elapsed, the gases mixed again by diffusion; the portion next the sides moved towards the centre of the tube, and then another explosion could take place. Again waiting for this diffusion to do its work a second time, a third explosion was possible. According to this theory, the heat had nothing to do with the result, but only the influence of the sides. Now, to prove the correctness of my inference, I would refer to the experiments on dissociation of some Russian scientists, Menschutkin and Konswalow, which quite confirm my views; they found that if the vessels in which the experiments were made had roughened surfaces, or contained sand, asbestos, or ground glass, there was considerable dissociation. This just proves what I have said, that where there is a rough surface the combustion is more difficult, more sluggish, and cannot take place so readily. I wonder these gentlemen call this action "dissociation," as it would appear to be the influence of surfaces that brought it about. To some extent this is also corroborated by the evidence of the father of the dissociation theory, Deville. In his book he describes innumerable experiments which he made to prove dissociation by heat; but he says, at the same time, that he requires porous tubes. He has to employ material of various kinds, which he puts into the tubes he uses, and shows that much depends on the material he employs. Besides, there is a very curious circumstance which goes still further to prove what effect surfaces have. Deville proved dissociation of carbonic acid by producing carbonic oxide and oxygen; but he also dissociated carbonic oxide itself, producing carbonic acid and carbon. So that two directly opposite effects are supposed to be produced by the same means—viz., by the action of heat; but it seems quite impossible to conceive how heat could at one time produce dissociation of one kind, and at another time of another kind; at one time dissociate carbonic acid, with the production of carbonic oxide and oxygen, and at another time dissociate carbonic oxide, and make carbonic acid and carbon. There are other scientific men who have experimented on dissociation; for instance, Victor Meyer, who dissociated water by dropping melted platinum into it. Melted platinum has a high temperature, and he thus produced steam which was dissociated into oxygen and hydrogen. But the influence of platinum on hydrogen is well known; it condenses it, even so far as to set fire to it. Why, then, should it not be instrumental in bringing about dissociation? The same thing occurs, to a less degree, when steam is passed through a platinum tube heated from the outside. Meyer (who, by the way, disputes the correctness of Menschutkin's results) thus obtained signs of dissociation; but, of course, the heat inside the tube when heated in this way will not be very great, and the steam passing through the tube would not have much heat imparted to it, so that in this case the heat would be so small, that in spite of the presence of the surface of platinum, he only had traces of dissociation. He also obtained traces by passing carbonic acid through a platinum tube. It must not be forgotten that platinum has also an influence on oxygen; it is oxidised, and consequently it may be instrumental in bringing about this dissociation. It seems to me that it must be so, because the heat conducted to anything passing through a tube which is heated from the outside is so small in quantity and intensity as not to be sufficient in itself to produce dissociation; the heat, in fact, would be of a lower temperature than any flame we have. All that I have said tends to show, therefore, that in furnace building dissociation may be left almost entirely out of consideration. It is only in cases where furnaces have very small chambers, so that the flame comes in contact with surfaces, that the appearance of dissociation might take place. I have given you my theory of dissociation, and brought it into harmony with my system of furnaces, and I think I am right in my view. But I do not pretend to be an authority on scientific matters, and would rather leave these questions to scientific authorities to determine the right theory. Here (referring to a model) is a furnace which is supposed to be a portion of a boiler flue worked by radiation. The flame passes through the boiler tube without touching it, and gives its heat to the boiler by radiation. In returning, the flame has to play on the surface of the boiler, and heat it by contact. As long as the flame is in a state of active combustion, it should not touch any surface; but, after the combustion is completed, contact is necessary. I have such boilers at work; they do not make a particle of smoke, and there is a great saving of fuel, of the boilers themselves, and of furnace material. (Mr. Siemens then explained various diagrams, exhibited when his paper was read, showing the construction of furnaces; and also showed the regenerative gas stove, which he exhibited and described at the meeting of the Gas Institute in June last.) Mr. Siemens then went on to say that an English fire-place is a very healthy arrangement, as it gives out radiant heat only. The only drawbacks to it are the smoke produced and the mess of coal, ashes, and soot, which ought to be done away with. The stove exhibited in action is made with the view to increase radiation of heat; it is supplied with a regenerative burner producing intense heat, and consequently it has great heat radiating power. Gases which have passed through the combustion stage act in much the same way as air, which cannot radiate out heat if it is ever so hot, because it is not a solid body. I can give you the results of some experiments which I have made with my brother, Dr. Werner Siemens, who heated air to try whether it would radiate light, and it does not, even if ever so hot. If any light is radiated it is simply from particles of dust in the air, or some other solid matter which is suspended in it. These radiate heat, and emit light when heated, but air itself does not do so. In the gas-stove exhibited, the products of combustion going down behind the flame pass into a regenerator, which heats the air that is brought up for combustion. The heat of the products of combustion is given up by contact to the regenerator, which heats the air by contact, and the hot air, in combining with the gas, produces the intensely-hot and bright flame which has such high heat and light radiating power, thus the remaining heat of the products of combustion is not lost, but is used to increase the heat radiating power of the flame. This stove burns something like 14 cubic feet of gas an hour, about the consumption of two to three ordinary burners, and for any moderate sized room this would be quite sufficient. If this stove were used it would entirely abolish smoke, and, being a gas-stove, there could be no ashes or soot produced.

The Chairman said the main practical question involved was how far radiant heat, or how far direct-acting flame was the more economical. On the other hand, there was the very interesting scientific question of the heat—the dissociation of flame; both of which points were well worthy of very careful consideration.

Professor Armstrong said he had been greatly interested in hearing Mr. Siemens's remarks on the subject of dissociation, and felt very much inclined to take his side on this question. Attention had been specially called to the influence which surfaces had in promoting dissociation. He certainly agreed with Mr. Siemens in thinking that probably many of the results which had been obtained were not purely heat effects, but that the surface employed had a great deal to answer for. With regard to the experiments referred to, in the first place, one was inclined now to look at this matter from a somewhat different point of view from that taken by Bunsen, since the publication of Mr. H. B. Dixon's work, which had clearly shown that the combustion of carbonic oxide and the behaviour of carbonic oxide and oxygen mixtures were largely dependent on the amount of water vapour present; so that conclusions drawn from such experiments as those made by Bunsen were practically not of any great value. With regard to the Russian experiments referred to, there was also some doubt, as they had been altogether called in question by Victor Meyer. One would like to hear some discussion on the very important question raised as to the effect of incandescent solid matter. If he understood Mr. Siemens rightly, the whole radiant effect of a flame was due to the presence within it of incandescent solid particles. This certainly was a most important point. If the efficiency depended, as had been maintained, on obtaining the chief amount of heat-energy in this way, then the point to which attention had been called was of extreme importance; the method of heating described entirely depended on the acceptation of this theory. With regard to the beautiful burner exhibited, he could not help remarking that Mr. Siemens must do something before he introduced it into London. He must first help to get a better gas supply; for, practically, at the present time, they were not in a position to use such burners, because the Gas Companies supplied gas of such quality that the first thing it did was to close its own passage-way. The South Metropolitan Gas Company especially were, at the present time, engaged in trying to block up their own mains. They were using ordinary Newcastle coal alone, and the result was that the gas was highly charged with naphthalene, but contained nothing to prevent it being deposited; and, on cold weather coming on, it rapidly stopped up the main and service pipes.

Mr. SIEMENS said it was quite correct that, in his opinion, radiation only proceeded from the free carbon in the flame. If there was only carbonic oxide gas or hydrogen in the flame, there would be little radiation; but even these gases should be burnt in an open space before being brought into contact with surfaces to avoid loss of effect, as when flame acted directly on brickwork, combustion was very imperfect. What was called "dissociation" would set in; but, according to his views, it would be imperfect combustion. To work a furnace by radiant heat to the best advantage, fuel containing the largest proportion of hydrocarbons, or, in other words, gas from the coal generally met with, should be used.

Professor HUNTINGTON said this subject seemed to divide itself into two questions—the practical aspect, or the heat-value of Mr. Siemens's furnace, and the theoretical question as to where the maximum limit of heating power was to be found. With regard to the theory, he did not think it was worth very much. True theory could only be the result of perfect practice; and when they had the practice perfect it was time to formulate a theory. At present they had not sufficient results to enable them to formulate a theory; they could only frame working hypotheses to give direction to further investigation. With regard to the practical aspect of the question, it was difficult to carry on any discussion at present, because these furnaces, although being worked on a very large scale, were in the hands of Mr. Frederick Siemens and those associated with him; and, in order to discuss the matter, it would be necessary that they should be working in different places under different conditions, so that they might be compared. He could only say, in regard to the furnaces themselves, that he saw them working about a year ago at Landore, and was greatly struck with them. The first thing about them was their remarkable simplicity, which would give them a much longer life. The work could be carried on without stopping to make repairs. As far as he could judge, the temperature obtained in the Landore furnaces was all that Mr. Siemens claimed. All furnaces were heated partly by radiation and partly by contact. Mr. Siemens's furnace was an improvement in the direction of heating by radiation. With regard to dissociation, it always appeared to him that the results obtained by Bunsen and Deville were very far from finality. If he recollected rightly, they obtained the commencement of the dissociation for the aqueous vapour, for carbonic oxide, and for carbonic acid between 1100° and 1300° C.; but he had never yet heard that anyone had claimed to have ascertained the point at which complete dissociation took place. They had it on the authority of Deville that, when hydrogen was burnt in an equal volume of oxygen, he obtained 2500° C. under the conditions of the experiment; but this, so far as he (Professor Huntington) was aware, was the only statement on the point. No one had given anything conclusive to show the point at which the whole of the gases would undergo dissociation. In fact, Deville had pointed out that the tension of dissociation was to be defined on parallel lines with vapour tension, and that just as water would be converted into vapour at ordinary temperatures, so gases would undergo dissociation at a point very much below the temperature which would be obtained by the combustion of the constituents. It was evident, therefore, that the subject was quite an open one.

Professor ARMSTRONG said it was very interesting, in connection with this subject, to recollect that it had been shown that at a temperature not far below 2000° C., carbon dioxide did not undergo any change, whereas carbon monoxide decomposed to an appreciable extent, and water also was decomposed below this temperature.*

Dr. SQUIRE said Mr. Siemens had called attention to the fact that the surface in a tube over which gases were passing favoured dissociation. He would remind him that surface in the same way favoured combustion. It was very well known that certain substances (such as oxide of iron, clay, and some others) would promote the combustion of particular gases; so that there was at the same time a cause which would produce dissociation and also combustion. With regard to the temperature, for example, at which oxygen and hydrogen were dissociated, one could hardly expect to fix this, because, in a great many cases, the presence of the gas affected the decomposition of the material. He had himself found that it was perfectly impossible to saturate ordinary air with sulphur; as the two could not be made to combine. A certain percentage of sulphurous acid could be arrived at; but one could not get any further, however high the temperature was raised. But the presence of other gases very much affected the tendency to combine, or the decomposition of the substance in question. If gases acted more or less as vacua to one another, then also the products of decomposition, as well of decomposition going on, would materially affect the temperature of dissociation. One obtained a certain result, and could not go any further at this temperature. If the temperature went a little higher, there was a sort of partial decomposition. He would suggest to Mr. Siemens whether it was absolutely certain that his theory of the effect of contact was absolutely proved under these circumstances.

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* *Note added during revision by Dr. Armstrong.*—I find that I overstated the temperature, which was only about 1700°. It is noteworthy, however, that, whereas, according to Meyer and Langer, carbon dioxide passes unchanged through a *platinum* tube at this temperature, Meyer and Züblin have confirmed Deville's observation that it is decomposed to an appreciable extent at 1300° when passed through a *porcelain* tube filled with fragments of porcelain.

Mr. E. Dowson said the question of heating boilers in this way by gas was very interesting, and the chief point appeared to be that the flame should not be allowed to come in contact with the boiler-plates, because this lowered the temperature by reason of the contained water; and they were told that this imperfect combustion and consequent loss of heat might be avoided by not letting the flame come in contact with the plates, and by using only the radiating power of the flame and the conducting power of the products of combustion. In principle this was doubtless correct; and the more nearly it could be followed in practice, the less loss of heat would there be, and the less nuisance from smoke. At the same time it must be remembered that the exigencies of construction and space required the greatest heat to be produced with a minimum surface exposed; and he had some fear that such a system as this could hardly be applied with advantage where such a concentration of heat effect was necessary. At any rate, he should be glad to hear Mr. Siemens's views on this point. Where a boiler was heated by gas drawn from a producer, there was no incandescent mass of fuel, but only the flame and the products of combustion to heat the boiler with; and it was, therefore, no doubt of the greatest importance that the best possible treatment of the flame should be correctly understood. They had to seek not only a flame of high radiating power, but also one capable of giving up its heat by conduction rather than by radiation. With this point in view, he would ask Mr. Siemens if he would kindly state whether he had obtained any economic advantages by applying his system to a boiler of ordinary construction. It would be extremely valuable if he had data derived from experiments on one boiler with the three systems of firing to which he had referred—(1) heating with solid fuel, (2) heating with gaseous fuel with contact, and (3) heating with gaseous fuel without contact.

Mr. Siemens said so many questions had been asked that he could hardly answer each separately. He could only say that for the last ten years he had used no other fuel than gas, and that the results obtained by the new method of heating are to be compared with those formerly obtained by the old method of heating by gas. There was always a great advantage in using gas as fuel if it were well managed; there was not only the saving in fuel, but also a very great saving in labour. There was also great economy in the wear and tear of furnaces and material of all kinds, but if they worked with gas they required to use it constantly. If it was employed in the daytime and not at night, there would be a loss, but if there was application for the gas both during day and night, it was very economical. He only used radiant heat as long as the flame radiated out heat; as soon as the visible flame ceased he brought the gases into contact with the surfaces he wanted to heat, for when combustion ceased, then the gases might be brought into contact with anything as they had no injurious effect. It was only the active flame which injured surfaces, not by its heat, but by the excessively rapid motion of the gases. Surfaces upon which a flame played were always roughened, and he took this to prove the destructive action of the flame which no material could stand for any length of time. In attempting to use an active flame, and to abstract the heat by contact, a double injury was inflicted; the heat that might otherwise be produced was lowered, and the material with which the flame was brought into contact was destroyed, besides smoke being produced. In using a flame which had little or no radiating power in it there was the same need for a combustion chamber, for the flame should not be allowed to play at once against surfaces or brickwork, as then there would be imperfect combustion. It was the surface itself, not alone its cooling influence, which was injurious to the flame. In heating a boiler it was still necessary, even if they had fuel which did not radiate heat, to provide a combustion chamber.

Mr. Fletcher said there could be no doubt about the efficacy of the radiant heat produced from combustion; for all furnaces were, as a rule, heated entirely by radiant heat. He thought, therefore, they would all support Mr. Siemens's view that it was radient heat after all, which was employed generally when a very high temperature was required. It was chiefly where a low heat was required (such as in a boiler) that the heat of conduction was mainly used. If Mr. Siemens had made experiments on any particular fuels, and could tell the meeting the difference between the heating power of fuel used where the bottom heat only was employed, and where it was so burnt as to develop more particularly the radiant heat, and could give them some figures showing the amount of water evaporated per 1lb. of coal burnt under the two systems, it would, he thought, be very valuable.

Mr. Siemens said he had not paid very much attention to the subject of boilers, and could not give any figures. He had fired his boilers on the new system because he was convinced that it would be a great advantage, and found it was so, for he used from 20 to 30 per cent. less fuel to do the same work as before; and the boilers worked more regularly, had a longer life, and required much less attention. With regard to heating by radiation in chemical furnaces he had already stated that there were cases were radiant heat was used for some particular purposes; but it had been explained in these cases why it was so used, and even then, such furnaces did not work fully by radiation as he had explained it. In the way furnaces were generally constructed the flame struck against anything, and this was just what should be avoided.

Mr. Mactear said it occurred to him that there was one furnace especially in which the principle of radiation was carried out very fully—viz., the old-fashioned regulus furnace which existed on the Tyne as far back as 1822. In this furnace the space above the main part to be heated was very high, but the bridge also was very high, and had a narrow slit, and the flame came over. Being a very small body of flame compared with the mass of the furnace, it floated about almost like a cloud. There was very little draught in the furnace, and the heating was practically all done by radiation from the flame. Mr. Siemens seemed to have taken this principle, and expanded it enormously; and there was no doubt the results he had obtained were of very great importance. He (Mr. Mactear) thought, however, that a good deal of this was due to one fact which had not been touched upon, and that was that for a given consumption of fuel he had enormously increased the size of the combustion chamber. Consequently, the speed of the passage of the gases through this chamber was much retarded, and a longer time given for the combustion of the gas in the chamber itself. This seemed to him a very important matter, which tended much to the success of the system. With regard to Bunsen's experiments on dissociation, if the question depended, as Mr. Siemens thought (and probably with reason), on the effect of surfaces, and if the combustion of a gas ceased at a certain point, owing to the amount of surface exposed, it would seem a comparatively easy matter to repeat the experiments with tubes of different diameters, and see whether the amount of dissociation differed. This would probably show the effect of surface contact.

Mr. Siemens said Mr. Mactear spoke in the same way as Mr. Deville had done. He obtained dissociation, but he wanted all kinds of surfaces to bring it about; and as long as surfaces were wanted there was no saying what influence they had. With regard to the heating of boilers, he did not think the production of steam would be less with radiant heat, but rather more; but if the flame struck against the sides or other surfaces a considerable amount of heat would be lost, because they would get covered with soot, which the heat could not penetrate. Radiant heat, especially with a large body of flame, had always a very powerful effect, because flame radiated heat from the inside as well as from the outside, not like a solid body, which only radiated from its surface. The slowness with which the flame passed through the furnace chamber had a very beneficial effect, as Mr. Mactear had stated, as it allowed the radiant heat more time to act, whilst as regards the heating by contact which took place afterwards, this should be more effective than in an ordinary solid fuel furnace, on account of the higher initial temperature of the flame.

Mr. Dowson said he thought Mr. Siemens had somewhat modified his original proposition. It now appeared to be that the extra space given in the new type of radiation furnace over the space required on the old type of furnace was only necessary until such time as the combustion of the gas should be complete.

Mr. Siemens said that the combustion of the flame should be completed within the heating chamber of the furnace; the resulting hot gases should then be passed into the regenerators where they would give up their remaining heat by contact. In the combustion chamber the flame worked by radiation only; by the heating chamber he meant the combustion chamber in the case of a furnace, and by the flame-flue is meant the combustion chamber of a boiler.

NOTES ON THE CHEMISTRY OF SOAP.

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It has long been known that when common salt is added to a hot aqueous solution of potassium stearate or oleate, or of both salts mixed, together with larger or smaller quantities of analogous potash salts (such as the products of saponification of tallow, etc.), double decomposition takes place, resulting in the formation of potassium chloride and the soda soap corresponding with the potash soap originally employed; and further, that when neutral potassium or sodium stearate is treated with large masses of water it is partially broken up or *hydrolysed*, forming caustic alkali and an acid stearate, a similar behaviour being also noticeable in the case of analogous salts or soaps containing other fatty acids—*e.g.*, palmitic. We have recently had occasion to study somewhat in detail these and other allied phenomena, and have obtained results some of which it may be of interest to put upon record.

I.—*On the Relative Affinities shown by Alkalis for certain Inorganic and Organic Acids in presence of each other.*

The reaction of sodium chloride upon potassium stearate (or other analogous salt) above referred to has long been utilised for the purpose of obtaining hard soda soaps from soft potash soaps, in localities where the relative cost of potash and soda as saponifying agents has made it more convenient to employ the former alkali to react on the fatty matters used. It is usually regarded as an example of a general rule obtaining in such cases, and also employed in the calculation of analyses of mixed saline matters (such as the salts dissolved in a mineral water), which rule may be thus stated: When no interfering causes (such as relative insolubility) are at work, *the stronger alkali is associated with the stronger acid*, the order of alkaline strength being (1) potash, (2) soda, (3) ammonia. In the case of common salt and a potash soap, the stronger acid hydrochloric acid (which may be regarded as contained in the salt solution) in accordance with this rule tends to associate itself with the stronger base potash, whilst the weaker alkali soda combines with the feebler fatty acids of the soap. Similarly when nitrate of soda and carbonate of potash are brought together, nitrate of potash and carbonate of soda result, the stronger acid nitric acid combining by preference with the potash; this reaction has long been utilised as a means of producing saltpetre from nitrate of soda. The result of our experiments, however, is to show that this rule does not express the whole truth quite correctly. We find that when two alkalis and two acids are simultaneously present, there is a partitioning of each pair, giving rise to four distinct salts, the relative quantities of which depend to some extent upon extraneous circumstances, and are largely influenced by the relative masses of the two alkalis on the one hand, and of the two acids on the other. In certain cases, when potash and soda are the alkalis, the salts formed by the combination of potash with the stronger acid and soda with the weaker one, very greatly exceed in amount the other two salts formed by the combination of potash with the weaker acid and of soda with the stronger one; but by varying the conditions, and more especially the relative masses, these relationships may be greatly modified, and even inverted. For instance, sodium chloride and potassium stearate, when intermixed in such proportions relatively to the water present that the soaps formed are thrown wholly out of solution in the brine, will give rise to a mixture of potassium and sodium stearates as insoluble curd and of potassium and sodium chlorides in the brine, of which salts the second and third contain respectively the majority of the stearic acid and of the potash. But, on the other hand, if potassium chloride and sodium stearate be similarly intermixed in suitable proportions, the soap thrown out of solution will contain the majority of the stearic acid combined with potash instead of soda, and the majority of the soda will be found as chloride in the aqueous brine instead of in the soap, the action indicated by the rule of "stronger acid and stronger base" being thus apparently inverted.*

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* In connection with the proportions in which the acids and bases of the two admixed salts will partition themselves so as to form four salts, it may be noticed that the reaction of potassium carbonate upon sodium nitrate really gives rise to four salts, and not to two only, the nitric and carbonic acids being both divided between the potash and soda respectively. That this is so is evident from the circumstance that if sodium nitrate and potassium carbonate in equivalent proportions be mixed, dissolved in water and

In the same kind of way, if caustic potash and caustic soda be mixed and brought into contact with a fatty acid, the latter being in quantity insufficient to neutralise the total alkali present, the rule of "stronger base and stronger acid" would suggest that potash soap would be formed to the exclusion of soda soap, were enough potash present to saturate the fatty acid. In point of fact, however, the product of the action is a mixture of potash and soda soaps, with excess of caustic potash mixed with caustic soda, where the ratio between potash and soda in the combined condition is sensibly the same as that of the uncombined alkalis, and consequently the same as that of the alkalis originally employed; so that in such a case the affinities of the two alkalis are sensibly equal. It is remarkable that practically the same result is obtained with most of the ordinarily occurring fatty acids, the nature of the acid not materially influencing the result. Thus the following numbers were obtained in a series of experiments with pure stearic acid; pure oleic acid; a mixture of stearic and oleic acids, etc., obtained by decomposing a tallow soap with hydrochloric acid; a similar mixture of nearly equal amounts of stearic, palmitic, and oleic acids, obtained from a palm-oil tallow soap; and the crude lauric acid (mixed with lower and higher homologues) similarly obtained from a cocoa-nut oil soap. In each instance the quantities of materials used were, for a given amount of fatty acids, exactly the neutralising amount of caustic potash together with the equivalent amount of caustic soda, so that two equivalents of total alkali were present to one of fatty acid. The alkaline leys being weighed out, the weighed quantities of fatty acids were added, and the whole heated to near 100° in a water bath, without stirring, until the fatty acids were melted, forming a supernatant layer. The whole was then briskly agitated for some time, and finally left at rest (after concentration by boiling down in the case of the cocoa-nut acids), the amount of water present being such that the resulting soaps were wholly insoluble in the alkaline ley; it sufficed to draw off and analyse a portion of the fluid, to obtain the data for calculating what fractions of the alkalis used were respectively converted into soap and left uncombined.

FATTY ACID EMPLOYED.	PERCENTAGE OF TOTAL FATTY ACID CONVERTED	
	Into Soda Soap.	Into Potash Soap.
Pure Stearic Acid..	51·2	48·8
Pure Oleic Acid..	50·8	49·2
Crude Stearic and Oleic Acids (tallow)........................	51·5	48·5
Crude Stearic Palmitic and Oleic Acids (palm oil and tallow)	48·2	51·8
Crude Lauric Acid (cocoa-nut oil)	49·7	50·3
	Mean .. 50·3	49·7

These figures clearly show that for all practical purposes the potash and soda existed in every case half as soap and half as uncombined hydroxide in the mixed mass resulting from the mechanical incorporation of molten fatty acid and two equivalents of caustic alkali, half as potash and half as soda.

It should hence result that if a soda soap be fused and then well intermixed with as much caustic potash as is equivalent to the soda present, or if a potash soap be similarly treated with the equivalent quantity of caustic soda, the final product should be the same in each case—viz., a soap containing half the fatty acids combined with one alkali and half with the other, and an alkaline ley containing equivalent amounts of potash and soda. In point of fact, a close approximation to this result is actually attained in practice. Owing to the mechanical difficulty of obtaining thorough incorporation in experiments with small quantities made in the laboratory, the amounts of potash displaced by soda, or *vice versa*, usually fell a little below 50 per cent., but approached sufficiently near to that figure to indicate that it would have been arrived at with more complete means of admixture. Thus the following numbers were obtained with the tallow-soap acids above referred to, (*a*) when exactly neutralised with potash, and then stirred well with the equivalent amount of caustic soda solution; (*b*) when exactly neutralised with soda and then stirred with the equivalent amount of caustic potash solution—

(*a*) Percentage of Potash Soap converted into Soda Soap	(*b*) Percentage of Soda Soap converted into Potash Soap
48·8	46·0

Similar results were obtained with other fatty acids.

Most remarkably-different results were obtained in a parallel series of observations in which the carbonates of potash and soda were used instead of the hydroxides respectively. In these experiments various fatty acids were exactly neutralised either by caustic potash or soda, as the case might be, and were then well intermixed with aqueous solutions of the carbonate of the other alkali. Finally the mass was dried on the water-bath and treated with strong (nearly anhydrous) alcohol, which dissolved out the soaps and left behind the alkaline carbonates, the reaction invariably giving rise to four products—viz., potash soap, soda soap, potassium carbonate, and sodium carbonate, but in relative proportions widely differing from those observed with the hydroxides—the amount of potash contained in the mass as soap being to the soda similarly contained in far greater proportion than that of the total masses of potash and soda present as carbonates; in fact, when quantities of potassium carbonate were intermixed with soda soaps in proportions not exceeding the amounts equivalent to the soda in the soaps, by far the majority of the potash passed into combination with the fatty acids, equivalent quantities of sodium carbonate being formed; whilst on intermixing sodium carbonate with potash soaps, even in quantities largely in excess of the amounts equivalent to the potash present, only a comparatively small percentage of fatty acids became combined with the soda, forming an equivalent amount of potassium carbonate. Thus the following table exhibits side by side some of the numbers obtained, (*a*) on intermixing

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evaporated to dryness, the solid residue is always hygroscopic, or even deliquescent, indicating the presence of more or less unchanged potassium carbonate. Similar results are observed with chloride instead of nitrate.

soda soaps with carbonate of potash, (*b*) on intermixing potash soaps made from the same fatty acids with carbonate of soda. The fatty acids employed were the same mixtures of stearic and oleic, of stearic palmitic and oleic, and of lauric and homologous acids described above, together with the crude ricinoleic acid obtained by saponifying refined castor oil with caustic soda and decomposing with an acid—

FATTY ACIDS EMPLOYED.	(*a*) Soda Soaps fused with K_2CO_3. Percentage of total Fatty Acids present.		(*b*) Potash Soaps fused with Na_2CO_3. Percentage of total Fatty Acids present.	
	Equivalent to the K_2CO_3 added.	Actually converted into Potash Soap.	Equivalent to the Na_2CO_3 added.	Actually converted into Soda Soap.
Stearic and Oleic (tallow)	10·1	8·0	—	—
Ditto	45·7	31·1	—	—
Ditto	100·0	97·95	100·0	4·3
Ditto	101·2	99·0	1000·0	15·0
Stearic Palmitic and Oleic (palm oil and tallow)......	57·2	52·1	—	—
Ditto	108·0	90·8	177·0	9·5
Crude Lauric Acid (cocoa-nut oil)	52·8	46·1	—	—
Ditto	111·8	87·9	197·0	6·2
Crude Ricinoleic Acid (castor oil)	50·0	48·1	—	—
Ditto	100·0	93·8	205·0	8·2

These observations throw some light on the effect well known to be produced on the texture and grain of certain kinds of soda soaps when melted and stirred with pearlash solution. The hard soda soap is partially converted into soft potash soap, thus modifying the texture, whilst the carbonate of soda formed as complementary product exerts a "closing up" or hardening action on the mass, avoiding the pastiness and softness that might possibly be caused by the presence of the potash soap when produced in any quantity. It is, however, to be remarked that inasmuch as no neutralisation of the extra alkaline matter thus added takes place, soaps thus subjected to "pearlashing" must necessarily contain "free alkali" to an extent proportionate to the amount of pearlash added, and hence may readily be rendered most objectionably alkaline and deleterious to sensitive skins when this process is adopted. Quantities of free alkali, equivalent to 10 or 12 per cent. of that present combined as soap, are not infrequently met with in "pearlashed" soaps, an amount entirely inconsistent with high quality from the point of view of suitability for application to tender skins.—*Vide* Section III.

It is thus evident that when fatty acids and carbonic acid on the one hand, and potash and soda on the other, are simultaneously present, the prevailing tendency is towards the formation of potash soap and carbonate of soda. Quite the opposite result is brought about if chlorides be substituted for carbonates, the prevailing tendency now being towards the formation of soda soap and potassium chloride; at least these salts are formed in preference to potash soap and sodium chloride when the relative masses of the two alkalis are nearly equal, although by modifying the relative masses the latter pair of salts can be made to be generated in preference to the former pair. Thus the following figures were obtained in a series of experiments carried out in such a way that the total potassium and sodium present were in equivalent proportions. A known quantity of fatty acids was divided into halves, and each half converted into neutral soap by addition of caustic potash to the one and caustic soda to the other. The two soaps were then mixed and dissolved in hot water (150 molecules H_2O to 1 molecule of mixed soap—*i.e.*, to ½ molecule of potash soap + ½ molecule soda soap), and treated at a temperature not far from 100° C. with a mixture of equivalent weights of potassium and sodium chlorides (20 molecules mixed chlorides, or 10KCl+10NaCl) in the state of powder, poured in with continuous agitation kept up for some minutes. The curds that separated were collected after complete cooling (the proportion of water and saline matter being such that no soap then remained dissolved) and well pressed in calico, and finally were dried in the water-bath and boiled with strong alcohol. Small quantities of chlorides were thus taken into solution as well as the soaps, but as the chlorine thus present only represented an amount of alkali equivalent to from 2 to 5 per cent. of that equivalent to the fatty acid dissolved as soap, the error thus introduced was not sufficiently great to affect the general result, which was uniformly this, that between 1½ and 6 molecules of soda soap were contained in the alcoholic solution for 1 of potash soap. Thus with various of the fatty acids and admixtures thereof above described the figures given in the first table on page 628 were obtained, exhibiting considerable variation with the nature of the fatty acid, but always such as to indicate formation of soda soap to a notably greater extent than of potash soap.

In other series of experiments the quantity of sodium present was increased relatively to the potassium, under which circumstances, as might be expected, still larger proportions of soda soap were formed. On the other hand, increasing the amount of potassium present relatively to the sodium, decreased the amount of soda soap and increased that of potash soap until the latter predominated. Thus the figures given in the second table on page 628 were obtained in some experiments in which (*a*) potash soaps dissolved in M molecules of water were salted out by adding N molecules of sodium chloride for 1 molecule of soap; (*b*) soda soaps dissolved in M molecules of water were salted out by adding N molecules of potassium chloride for 1 molecule of soap.

It is thus evident that in the (*b*) series of experiments the soap thrown out of solution was essentially potash soap*—a conclusion borne out by its consistency. It is noticeable, on comparing the figures in the (*a*) and (*b*) columns for each kind of fatty acid, that the tendency to convert potash soap into potassium chloride and soda soap by the action of sodium chloride, is *cæteris paribus*, distinctly more marked than the tendency to convert soda soap into sodium chloride and potash soap by the action of potassium chloride, precisely as might be inferred from the above experiments with equal molecules of potash and soda compounds.

A number of experiments were also made on the mutual reactions of ammonium chloride upon potash and soda soaps, and of potassium and sodium chlorides upon ammonia soaps. In accordance with the rule of "stronger acid and stronger base," it might be anticipated that on adding ammonium chloride to a potash or soda soap the chlorine will combine with the fixed alkali metal, an ammonia soap being the complementary product; the experiments just described, however, would suggest that under certain conditions this action might be inverted, so that on adding potassium or sodium chloride to an ammonia soap more or less potash or soda soap might be formed, together with the equivalent amount of ammonium chloride. In point of fact this is the case; whilst almost complete displacement of fixed alkali from combination with fatty acid ensues when a potash or soda soap is dissolved in 200 molecules of water, and after cooling well stirred with twenty molecules of ammonium chloride; the opposite effect is produced when twenty molecules of potassium (or sodium) chloride is similarly stirred into a cold solution of an ammonia soap in 200 molecules of water, the majority of the fatty acids becoming transformed into potash (or soda) soap, and a corresponding amount of ammonium chloride being formed. It is remarkable in this connection, moreover, that in two experiments, alike in all respects save that potassium chloride is used in the one and the equivalent quantity of sodium chloride in the other, a perceptibly larger proportion of fatty acids becomes converted into soda soap than does into potash soap, indicating a greater tendency for potash to remain as chloride than for soda. Thus the numbers given on page 629 were obtained in four series of observations, the numbers in the four last columns respectively indicating the percentages of total fatty acids employed as soaps converted—

(a) From the condition of ammonia soap to that of soda soap by salting out with sodium chloride;

(b) From the condition of ammonia soap to that of potash soap by salting out with potassium chloride;

(c) From the condition of soda soap to that of ammonia soap by salting out with ammonium chloride;

(d) From the condition of potash soap to that of ammonia soap by salting out with ammonium chloride.

FATTY ACID USED.	PERCENTAGE OF FATTY ACID CONTAINED		Molecular Ratio of Soda Soap to Potash Soap.
	As Potash Soap.	As Soda Soap.	
Pure Oleic Acid	38·0	62·0	1·63 to 1
Crude Ricinoleic Acid (from castor oil)	17·8	82·2	4·6 to 1
Stearic, Oleic, and Resin Acids, mixed (primrose soap)	17·2	82·8	4·8 to 1
Crude Lauric Acid (from cocoa-nut-oil soap)	15·1	85·9	5·7 to 1

FATTY ACID USED	M	N	(*a*) Potash Soaps salted out with NaCl. Percentage of Fatty Acid in Curd.		(*b*) Soda Soaps salted out with KCl. Percentage of Fatty Acid in Curd.	
			As Potash Soap.	As Soda Soap.	As Potash Soap.	As Soda Soap.
Stearic and Oleic Acids (tallow	100	5	10·5	89·5	79·1	20·9
Ditto	200	20	5·1	94·9	82·1	17·9
Stearic Palmitic and Oleic Acids (palm oil and tallow)	200	20	3·8	96·2	95·8	4·2
Crude Lauric Acid (cocoa-nut oil)	200	20	5·1	94·9	74·8	25·2

The ammonia soaps used for series *(a)* and *(b)* were prepared by just fusing the fatty acids and then well incorporating moderately concentrated ammonia solution in quantity slightly in excess of the amount equivalent to the fatty acids, and finally dissolving in water so as to obtain nearly clear solutions; those obtained in series *(c)* and *(d)* invariably decomposed spontaneously during preparation, giving off more or less free ammonia and forming acid ammonia soaps.

On the whole, all the experiments tend to show that when a soap and a neutral salt containing an alkali different from that in the soap are brought together, four salts are formed in relative proportions, largely governed by the relative masses of the two alkalis and the two acids; but there is always a more or less marked tendency for the stronger base and stronger acid to associate themselves together in proportions greater than those deducible from their actual masses relatively to the weaker base and weaker acid. When the salt is carbonate, since carbonic acid is a weaker acid than the fatty acids of soap, the prevailing tendency is (when the alkalis are potash and soda) to form potash soap and sodium carbonate; and when the salt is chloride, since hydrochloric acid is a stronger acid than the fatty acids of soap, the prevailing tendency is to form potassium chloride and soda soap. But these tendencies can be very largely overcome by the action of mass, especially with chlorides.

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* This conversion of soda soap into potash soap by salting out with potassium chloride does not appear to have been hitherto noticed; at least we have been unable to find any printed record of the observation amongst the voluminous literature relating to soap that has come under our inspection.

FATTY ACID USED.	Ammonia Soap salted out with— (a) NaCl	(b) KCl	Ammonium Chloride added to— (c) Soda Soap.	(d) Potash Soap.
Stearic and Oleic Acids (tallow)	91·3	71·8	99·0	99·5
Ditto (another specimen)......	—	70·7	—	99·3
Stearic, Palmitic, & Oleic Acids (palm-oil and tallow)	90·3	61·6	99·5	99·7
Crude Lauric Acid (cocoa-nut oil)	86·0	—*	99·5	99·5
Crude Ricinoleic acid (castor-oil)	66·0	—*	99·7	99·5

It is noticeable, on comparing the figures in the (*a*) and (*b*) columns, that the tendency of soda to pass from the state of chloride to that of soap is, *cæteris paribus*, more strongly marked than the similar tendency in the case of potash, precisely as might be anticipated from the previous results.

* The potash soaps of cocoa-nut and castor-oils formed could not be separated from the brines so as to permit of analysis.

On the other hand, when only fatty acids are present, and no mineral acid or carbonate, there does not appear to be any marked tendency for either potash or soda to combine with the acid by preference.

	Percentage of NH_3 calculated as dry substance. Stearic.	Lauric.	Oleic.	Ricinoleic.
Freshly-prepared, still moist	6·05	8·30	6·00	5·75
After 2 weeks...... ...	5·00	5·67	—	—
„ 13 „	3·62	1·06	2·52	2·67
„ 26 „	2·36	4·02	0·87	0·16
Calculated for NH_3,HX	5·65	8·02	5·69	5·51
„ $NH_3,2HX$	2·91	4·18	2·93	2·85

In connection with the spontaneous decomposition of ammonia soaps, the following observations may be of interest:—A number of ammonia soaps was prepared by thoroughly intermixing fatty acids just fused with excess of moderately strong ammonia water, the whole being then dried over sulphuric acid for some months; the mass was pulverised and stirred up occasionally when sufficiently dry, samples being drawn from time to time and submitted to analysis. In all instances it was found that a rapid loss of ammonia took place during the first few weeks, until the composition approached that of a diacid salt, $NH_3,2HX$ (where HX represents the acid), after which the rate of loss became much slower. The temperature during the period of standing over sulphuric acid was rarely as high as 18° C., the substances being kept in an unwarmed room during winter and spring: on further heating the partly-decomposed salts to temperatures not very elevated, a distinct evolution of ammonia took place; whilst after heating for a few hours to 100° C., practically all was expelled and nothing but fatty acids left. The above figures represent the progressive loss of ammonia observed with crude lauric acid (from cocoa-nut oil), pure stearic acid, crude ricinoleic acid (from castor-oil), and nearly pure oleic acid, when converted into ammonia salt containing a small excess of ammonia intermixed therewith; similar results were obtained in various analogous experiments with mixtures of acids. On the whole, it appears that the diacid ammonia salts of the saturated fatty acids (lauric and stearic) lose ammonia distinctly more slowly than the corresponding salts of the unsaturated acids (oleic and ricinoleic).

II.—*On the Amounts of Hydrolysis effected in Soaps with varying proportions of Water.*

The decomposition of neutral soaps by water into acid soaps and free alkali has long been known as a fact, but systematic measurements of the amount of decomposition effected under varying conditions with different kinds of soaps do not as yet appear to have been made. Having recently had occasion to study this point somewhat minutely, we have arrived at a number of figures capable of being expressed graphically as curves, and leading to the following general conclusions:—

(1.) The amount of hydrolysis brought about by the action of a given quantity of water on a neutral soap is variable with the nature of the fatty acid from which the soap is made, but in all cases increases with the amount of water employed relatively to the soap, but less rapidly; so that curves plotted with the amounts of water as abscissæ and the proportions of alkali set free as ordinates are concave downwards.

(2.) Addition of excess of alkali to a neutral soap causes a diminution in the amount of hydrolysis effected under given conditions, to such an extent as completely to stop the action with comparatively small proportions of water when the free alkali only amounts to a fraction—say 20—25 per cent.—of the alkali combined with the fatty acids.

(3.) Alcohol, even when not absolutely anhydrous (90—95 per cent.), does not decompose ordinary neutral soaps into free alkali and acid salts; if, however, water be added to an alcoholic soap solution, more or less hydrolysis is brought about; so that if a gelatinous mass of neutral soap dissolved in strong spirit (containing a little phenol phthalein) be treated with water, a more or less strongly-marked colouration is noticeable as the water diffuses into the mass. (This affords a good lecture illustration.)

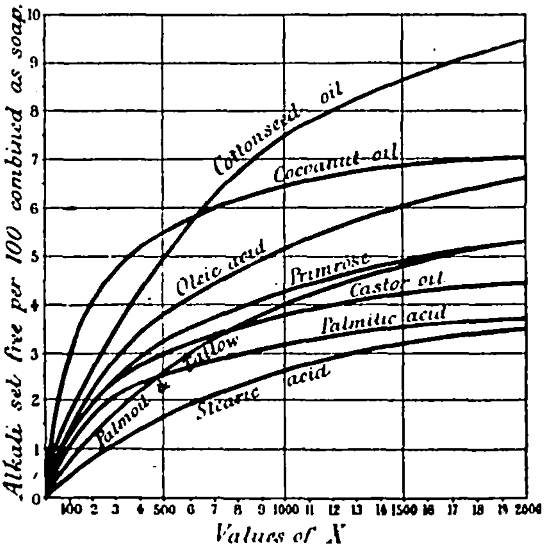
The experiments were made as follows:—The soaps examined were either prepared by ourselves or obtained from manufacturers with information as to the fatty acids present; they were carefully analysed, and in those cases where minute amounts of free alkali were present corrections were made for these small amounts.* Weighed quantities of soap representing known amounts of *anhydrous* soap were dissolved in known quantities of distilled water on the water-bath, and after cooling to near the ordinary temperature the liquids were treated with pure sodium chloride, so as to throw out of solution all, or nearly all, the soap as a curd. The curds thus precipitated, on drying and dissolving in alcohol, were always more or less acid, phenol phthalein being the indicator; so that one way of determining the amount of hydrolysis was to determine the quantity of alcoholic potash or soda solution requisite to neutralise this acidity. It was found, however, in practice to be far more convenient to determine the alkali contained in an aliquot portion of the brine, correcting the amount found for the free alkali originally contained in the soap (when any). With smaller proportions of water the addition of moderate quantities of salt sufficed to throw all soap out of solution so perfectly, that at most only traces of fatty acids could be obtained from the brine by acidulating and shaking with ether; with larger proportions it was found convenient to evaporate the brine until nearly saturated, and filter again from any soap thrown out of solution during the evaporation, using only a fraction of the salt requisite to saturate the water for the salting out, rather than to use more salt in the first instance and titrate without evaporation, greater accuracy in hitting the terminal reaction being thus attained. With cocoa-nut oil soap the brines were evaporated to dryness, and then treated with just sufficient water to dissolve the salt and filtered; in this way liquids free from more than traces of soap were obtained, the soap originally contained in the brine before evaporation being thus eliminated.

The following corrected mean values were obtained in a lengthy series of experiments with various soda soaps, all of which were either neutral or only faintly alkaline; the numbers represent the quantities of Na_2O set free by hydrolysis, reckoned per 100 parts of Na_2O contained in the soap combined with fatty acids, x molecules of water being used for one of anhydrous soap.

Fatty Acids.	Mean Molecular Weight.	Hydrolysis brought about by x Molecules of Water.				
		$x=150$	$x=250$	$x=500$	$x=1000$	$x=2000$
Pure Stearic Acid	281	0·7	1·0	1·7	2·6	3·55
Nearly pure Palmitic Acid	256	1·15	1·9	2·6	3·15	3·75
Crude Lauric Acid	195	3·75	1·5	5·4	6·45	7·1
Pure Oleic Acid	282	1·85	2·6	3·8	5·2	6·65
Crude Ricinoleic Acid	294	1·55	2·2	3·0	3·8	4·5
Chiefly Stearic, Palmitic, and Oleic acids (palm-oil tallow soap)	271	1·1	1·55	2·6	4·1	5·3
Chiefly Tallow and Resin (primrose)	280	1·5	2·2	3·1	4·2	5·3
Cotton Seed	250	2·25	3·0	5·0	7·5	9·5

The annexed diagram represents these values graphically, the value of X being abscissæ and the others ordinates.

It is noticeable, in connection with these figures, that the curves for stearic, palmitic, and lauric acids (cocoa-nut oil) lie in the order of the molecular weights of these three homologous acids, that for the acid of highest molecular weight underlying, and that for the acid of lowest molecular weight overlying, the third. It cannot, however, be hence inferred that the soda salts of acids of the fatty series are decomposed by water with the greater ease the lower their molecular weight, for Julius Thomsen's thermo-chemical investigations (*Thermochemische Untersuchungen*, i. p. 372) show that sodium acetate is not measurably broken up by water.



The curve for tallow-resin soap (primrose) indicates that the presence of a certain amount of resin soap in a mixture mainly consisting of sodium stearate and oleate does not materially affect the rate of hydrolysis, since the curve for this soap lies between the stearic and oleic acid curves.

The curves obtained with oleic acid and cotton-seed oil (which appears to contain some amount of an acid analogous to the linoleic acid of linseed-oil) overlie all the others, cocoa-nut oil excepted; ricinoleic acid, however, furnished a soap that hydrolysed markedly less readily than sodium oleate, though more readily than sodium palmitate and stearate.

The following numbers, obtained with some of the above soaps, illustrate the effect of the presence of free sodium hydroxide in diminishing the amount of hydrolysis produced by given quantities of water, the values being in all cases notably below those obtained as above with neutral soap solutions :—

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* For the determination of the free alkali the soap was dried and boiled with strong alcohol, and filtered hot; the filtrates were always neutral, showing absence of caustic alkali; the trifling residue of carbonate sometimes left on the filter was treated with standard acid in the usual way. *Vide infra*, Section III.

Nature of Fatty Acids used.	Extra Na_2O added to Solution, per 100 combined as Soap.	Hydrolysis brought about by x Molecules of Water.		
		$x=150$	$x=250$	$x=2000$
Crude Lauric Acid (cocoa-nut)	11·0	1·1	1·6	2·0
Cotton-seed Oil Soap	15·0	nil.	nil.	0·5
Stearic and Oleic (tallow)	20·0	nil.	nil.	nil.
Tallow Resin (primrose)	15·0	nil.	0·1	1·3

Similar results were obtained in numerous other experiments of the same kind.

III.—*Comparative Results yielded by Different Processes in ordinary use for the Determination of "Free Alkali" in Soap.*

In connection with this subject the following notes may be of some interest, being obtained as the result of a large number of comparative analyses made for the express purpose of testing the accuracy of the various methods. Three methods were examined, which for convenience of reference may be designated respectively as the "Alcohol test," the "Fatty Acid titration test," and the "Salting-out test" (or A.T., F.A.T.T., and S.O.T.). The first consists of the following operations :—The soap is dried, and then boiled with a sufficient amount of strong alcohol—or, better, absolute alcohol. The filtrate and washings are then titrated with alcoholic acid or alkali, using phenol phthalein as indicator, so as to obtain the positive or negative alkalinity of the solution (*negative* alkalinity indicating free fatty acid or diacid salt). The residue on the filter, consisting of carbonates, etc., is then titrated as usual, and the total alkalinity taken as the algebraic sum of the two valuations.[*] A number of direct experiments showed that if a perfectly-neutral soap were taken to start with, dissolved in alcohol, and added to a known quantity of alcoholic potassium or sodium hydroxide, the amount of alkali present was still correctly determinable with phenol phthalein as indicator, the presence of the soap introducing no marked error; whilst, on the other hand, if the same soap were dissolved in water and a known amount of sodium carbonate added, this amount was always sensibly indicated by the alkalinity of the residue obtained by evaporating to dryness, treating with alcohol, and filtering and washing thoroughly; whence it may be inferred that the A.T., when properly applied to a soap, gives accurate information, not only as to the total "free alkali," but also as to the proportion thereof existing as hydroxide and carbonate respectively. Accordingly, this process may fairly be taken as a standard of comparison for the others.

The second process (F.A.T.T.) consists in determining the total alkali present (that existing as soap together with the free alkali), separating the fatty acids in any convenient way, and finally dissolving these in alcohol and titrating with alcoholic potash or soda with phenolphthalein (or other suitable colouring matter) as indicator. Although at first sight this process would seem likely to be exact, yet in our hands it seemed almost invariably to be affected by a sensible error of uncertain direction.

On testing absolutely-neutral soaps in this way, numbers were obtained sometimes indicating a small positive excess of alkali, and sometimes a small negative one (excess of fatty acids) ; and in the same way, soaps that contained small or moderate amounts of free alkali when tested by A.T. often yielded distinctly different results when tested by F.A.T.T., the difference being sometimes + and sometimes −. As range of variation, from + 3 per cent. of the alkali present as soap to − 3 per cent. of the same usually represented the extremes ; but occasionally still larger discrepancies were observed, whilst concordance within ± 1 per cent. by the two tests was the exception rather than the rule. That no particular systematic error in one direction obtained was evidenced by the fact that taking the average of a long series of duplicate tests with different soaps, the + differences and the − differences sensibly balanced one another. Thus the following figures may be quoted as illustrations, the numbers representing the amounts of free alkali found per 100 combined as actual soap—*i.e.*, if the total alkali were 8·5 per cent. of the soap itself, and the free alkali were 1·0 per cent., then the free alkali would be $\frac{1{\cdot}0}{8{\cdot}5-1{\cdot}0} = \frac{1{\cdot}0}{7{\cdot}5} = 13{\cdot}3$ per cent. of the combined, and so on.

Nature of Soap.	A.T.	F.A.T.T.	Excess of A.T. over F.A.T.T.
Cotton-seed Oil Soap	7·0	11·0	−7·0
Chiefly from Bleached Palm Oil	5·0	10·8	−5·8
Primrose	1·1	5·0	−3·9
Transparent Soap, made by Spirit Process, containing 10 per cent. of Sugar	0	2·0	−2·0
A Curd Soap (highly alkaline)	18·3	18·1	+0·2
A British Cold Process Soap..	7·1	8·8	+1·4
A Continental Toilet Soap	1·2	−4·1	+5·3
A British ditto	8·0	2·5	+5·5

The four extreme differences here tabulated (first and last pairs of samples) were unusually large, the differences ordinarily not exceeding ± 3·0.

Inasmuch as an uncertainty in the actual amount of free alkali present thus seems to exist when the F.A.T.T. is used, amounting to ± some per cents. of the alkali present as soap, it follows that the F.A.T.T., though convenient and useful in the examinations of various kinds of soap where a sharply exact estimation of the free alkali is not material, cannot be safely used for the purpose of deciding whether the amount of free alkali in, for example, a toilet soap falls beyond a small limiting quantity. Thus in following the classification suggested by one of us in the Cantor Lectures on Toilet Soap, delivered some months ago before the Society of Arts,* the F.A.T.T. is practically useless, inasmuch as it is not possible to affirm certainly by its means whether or not a given sample of soap contains less alkali in a free state than $\frac{1}{40}$th part (2·5 per cent.) of that present as soap. The test might indicate perfect neutrality when upwards of 2·5 per cent. of free alkali is present; or it might condemn a soap as passing beyond the limit of 2·5 per cent. when really the soap might be perfectly neutral. On the other hand, for the commercial valuation of the detergent power of strongly alkalised or silicated soaps where a large portion, or even the majority, of the alkali present is not combined with fatty acids, the F.A.T.T. is often extremely convenient, necessitating but little additional trouble after the fatty acids formed on decomposing with acid have been determined.

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[*] It is not impossible for a free fatty acid (or, rather, a certain proportion of diacid salt) to coexist side by side with an alkaline carbonate in a solid soap; for example, in a soap prepared by intermixing a free fatty acid with a strong alkaline solution.

The third process (S.O.T.) is usually carried out in much the same way as the experiments above described on Hydrolysis, and consequently is apt to be even more inexact than the F.A.T.T. A certain amount of liability to error could no doubt be avoided by uniformity in procedure—*e.g.*, by always taking such a quantity of water to dissolve the soap in as would represent approximately a constant number of molecules of water to one of soap; but even if sources of error through varying proportions between water and dry soap could be avoided, there would remain the objection that a widely varying excess of alkali is always present in the brine over and above that to be determined, owing to the hydrolytic action of the water; and it is impossible to apply any mean correction sufficiently accurate for use to obtain exact results, because not only is the hydrolysis due to a given amount of water variable with the nature of the fatty acids present, but also it varies for a given fatty acid with the amount of free alkali in the original soap, increase in alkalinity of soap diminishing amount of hydrolysis. The figures on next column may be quoted as illustrations, being some obtained in one series of comparative valuations in which for one molecule of dry soap approximately 150 molecules of water were constantly used (roughly speaking, about ten parts by weight of water to one of dry soap); the soap being dissolved by the aid of heat, the solution was cooled and treated with an amount of pure common salt equal to one-fifth the weight of water, the curdy mass being then filter-pumped, and the last portions of adherent alkaline brine washed out with a few ccs. of saturated salt solution. With soaps largely consisting of cocoa-nut oil salts, an additional error is apt to be occasioned by incomplete precipitation from solution of soap by salt, apart from the increased error due to larger relative hydrolytic action on this class of soaps.

Considerably larger discrepancies between the results of the two methods were observed when relatively larger quantities of water were employed, as, for example, when a solution of soap (two, five, or ten per cent.) was precipitated by adding to it twice its volume of saturated brine, a mode of working sometimes adopted by soap analysts. It is hence evident that whilst the S.O.T. is often very convenient for testing highly-alkaline soaps commercially (the hydrolytic action of water on such soaps being a minimum), it is not of much value for discriminating the small amounts of free alkali which at most are permissible in high-class toilet soaps, inasmuch as the probable error of a test by this method will often (as with the F.A.T.T.) exceed the maximum amount of free alkali compatible with classification as a first-rate article; whence, finally it results that the "alcohol test" is the only one of real utility in such cases; independently of which, as above stated, it gives information as to the causticity or otherwise of the free alkali in a way impossible with the other two tests.

NATURE OF SOAP USED.	A.T.	S.O.T.	Excess of S.O.T. over A.T.
Pure Cocoa-nut Oil Soap, not strongly alkaline	5·0	9·1	4·1
Another sample of ditto	2·8	5·8	3·0
A British Toilet Soap, largely made from Cocoa-nut Oil	1·3	5·8	4·5
A foreign Toilet Soap, largely made from Cocoa-nut Oil (neutral)	0	3·5	3·5
A high-class American Toilet Soap........................	1·8	5·4	3·6
A Toilet Soap largely made from lard (neutral)	0	3·2	3·2
A second-class ditto, chiefly made from tallow............	1·9	4·8	2·9
A Cotton-seed Oil Soap.........	7·2	9·8	2·6
A Tallow-resin (primrose)	1·1	3·0	1·9
A Neutral Castor Oil Soap	0	1·7	1·7
A Bleached Palm Oil Soap	5·0	6·4	1·4
A tolerably Alkaline Curd Soap	18·3	18·3	0
Pure Stearic Acid Soda Soap (neutral)........................	0	0·7	0·7

IV.—*On two New Patents connected with Soap Manufacture.*

In connection with the above-described results, it may be of interest to refer briefly to two recent improvements introduced by one of us. One of these is mechanical rather than chemical, being primarily intended to diminish the large amount of waste unavoidably caused in the ordinary process of cutting up and shaping large rectangular blocks of soap for the formation of tablets, owing to the production of scrap; this is effected by forcing the hot fluid soap (by means of a pump, hydraulic ram, screw, or other convenient means) through a system of cooling tubes so arranged that the soap issues therefrom as a series of streams of soap just upon the solidifying point, but sufficiently soft and plastic to weld together completely on further passing into a moulding tube, from which a bar issues at the far end of dimensions regulated by a movable mouthpiece—the various tubes being jacketted, and so maintained at the particular temperatures requisite for the particular kind of soap operated upon.† In this way a considerable saving of time, labour, working space and material is effected as compared with the ordinary system, inasmuch as a continuous production of bar goes on as long as the supply of molten soap lasts.

The other is a purely chemical process, devised for the purpose of getting rid of the *bête noire* of the toilet-soap maker—viz., excess of alkali present in forms other than true soap, usually as carbonate or hydroxide. In the manufacture of Castile soap a small quantity of ferrous sulphate is often added to the mass before cooling, for the purpose of producing the peculiar "mottle" of this class of soap;[*] the sulphuric acid of this salt converts a portion of the free alkali into sulphate, thereby diminishing the free alkalinity. It is obvious that this latter result would be equally brought about by using any other salt that would act on the alkali of the soap in the same way as ferrous sulphate; but inasmuch as a metallic oxide would also result, the use of such compounds is objectionable, because staining and discolouration of the mass would be apt to be brought about, as well as other inconveniences, owing to the presence of metallic compounds. By substituting an ammoniacal salt this difficulty is obviated[†]; the free alkali becomes a neutral salt corresponding with the ammoniacal compound employed, whilst the complementary product is ammonia (free, or as carbonate, in either case readily volatile) which escapes, part immediately, the remainder during the processes of cutting up and shaping, stamping, and exposure to air to dry and harden before boxing; even should a minute amount of ammonia be retained in the centre of the tablet, this is of no practical consequence, as the action of ammonia on the skin (as on woollen goods generally) is well known to be far less corrosive and injurious than that of an equivalent amount of fixed alkali, especially soda. This method of getting rid of free alkali is particularly applicable to "milled" soaps, inasmuch as the exposure to air in the form of thin ribbons, which necessarily takes place over and over again during the grinding together of the stock soaps and colouring matters, etc., ensures the complete volatilisation of the ammonia; so that only the faintest traces, or none at all, can be found in the finished tablets.

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* *First Grade.*—Soaps which are, if not actually neutral, at any rate so far devoid of free alkali that the amount of total alkaline matter present in forms other than actual soap does not exceed $\frac{1}{40}$th part (2·5 per 100) of the alkali present combined with fatty acids as soap.

Second Grade.—Soaps in which the free alkali, although exceeding $\frac{1}{40}$th part (2·5 per 100) of that combined as soap, does not overpass $\frac{3}{40}$th of that amount (7·5 per 100).

Third Grade.—Soaps in which the free alkali exceeds $\frac{3}{40}$th (7·5 per 100) of the alkali combined as true soap.

Journal Soc. Arts, xxxiii. 1124, Nov. 6, 1885.

† *Vide* Patent Specification No. 10,001, 1884.

A working model of the machine described, and specimen bars made thereby (natural size), were exhibited; as also specimens of remelted and milled toilet soaps de-alkalised by the ammonia process, which was also illustrated in operation on the small scale.

DISCUSSION.

The CHAIRMAN, in opening the discussion, said the authors had referred to "mass action" as applied to the chemistry of soap; in his opinion many other departments of industrial chemistry involved this principle as well.

Mr. LANT CARPENTER had been much struck by the authors' views upon "mass action." It might account to some extent for the differences observed by wool-scourers and other large users, in the behaviour of batches of soap precisely similar in composition so far as chemical analysis went.

Mr. SAMUEL HALL thought that more stress was often laid on the necessity for having no excess of alkali in soap than the question demanded.

Mr. MACTEAR could fully corroborate Mr. Lant Carpenter's remarks. He had found much difficulty in producing batches of soap of uniform strength, to suit makers of fine Scotch tweeds. Sometimes a soap completely destroyed the colour, though differing in constitution in no degree from a previous batch deemed quite satisfactory. He thought that the author's proposal to replace the free alkali by ammonia as likely to aid in securing uniformity in this direction. About ten years ago an apparatus for squirting soap had been brought before him by an American. But it differed in this respect from that exhibited—viz., in squirting the soap into a single large tube instead of into several small ones. The result was that it was almost impossible to cool the soap unless a very long tube was employed, or the action was stopped every now and then. The apparatus before the meeting seemed to have effectually surmounted this difficulty.

Dr. ALDER WRIGHT, in reply, said that he believed it was the practice for certain firms to remelt their soap with some palm oil or other easily saponifiable fat, in order to neutralise any excess of alkali. Many other methods for removing excess of alkali from soap had been proposed at various times, but all had more or less failed in certain cases. What Mr. Mactear had said in reference to the American squirting machine was quite new to him; he had been under the impression that his machine was the first of the kind. In reply to Mr. Hall, it is not the dissolving of soap in water which causes excess of alkali to be objectionable, but the rubbing of this irritant on the skin. It had been over and over again established that if more than two or three per cent. of the total alkali were present in the form of carbonate, this would be enough, if used day after day, to produce most uncomfortable irritation, especially on the tender skins of ladies and children.

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THE LIME PROCESS FOR THE PURIFICATION OF COAL GAS.

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PART I.

THE impurities in coal gas, after passage through the scrubbers, containing sulphur as a constituent, are of three distinct kinds: (1.) Hydrogen sulphide. (2.) Substances yielding hydrogen sulphide when passed over heated surfaces, especially finely-divided platinum. These consist principally of carbon disulphide and traces of hydrogen thiocyanate, and probably carbon oxysulphide; the presence of the first having been further demonstrated by the production of its compound with triethyl phosphine (Hofmann), and its extraction from lime purifiers (Leicester Greville). (3.) Substances not convertible into hydrogen sulphide under these conditions, but burnt into sulphurous and thence to sulphuric acid, as estimated by the Metropolitan Gas Referees' sulphur test. These two latter kinds are generally classified as "sulpher compounds." The recent researches of Victor Meyer and his pupils on thiophen (C_4H_4S), which was first isolated from coal-tar benzene, and the relationship between this substance and pyrroline, seem to indicate that it, or its homologues, are amongst the sulphur compounds of the third class. The writer is now investigating this point. The principal purifying agents, adopted for the removal of members of the first two classes are ferric oxide and calcium hydrate, either in the damp state or in the form of milk of lime. The reactions taking place under various conditions between (1) calcium hydrate and hydrogen sulphide, (2) between calcium sulphide and carbon disulphide, under various conditions, and (3) the changes and decompositions induced by various substances likely to be present in coal gas issuing from the scrubbers, are the subjects of the present paper.

Reaction between Calcium Oxide and Hydrate with Hydrogen Sulphide.

The nature of the chemical changes between these substances has been the subject of investigation by

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[*] Many so-called mottled soaps of modern make are prepared in quite a different way; these substances differing from the old-fashioned mottles in being heavily watered, and consequently requiring a different mode of treatment to ensure the mottled appearance.

[†] *Vide* Patent Specification No. 11,681, 1881.

Berzelius[*] and Rose; [†] but more recently by Divers and Shimidzu,[‡] Folkard,[§] W. Foster, and the writer.[‖]

Firstly, it has been observed that, at temperatures below 100° C., perfectly-dry hydrogen sulphide is not absorbed by perfectly-dry calcium oxide[¶]—a phenomenon perfectly analogous to the non-absorption of dry carbonic anhydride by dry calcium oxide. The explanation in both cases is probably similar. Thus, in the latter case a small proportion of the calcium oxide dissolves in the trace of water. The solution absorbs the carbonic anhydride to form calcium carbonate, which is precipitated, and thus removed from the sphere of action; leaving the water to take up another small quantity of calcium hydrate, and so *ad infinitum.* In the former case, the calcium hydrate is dissolved as calcium hydrosulphide, $Ca(SH)_2$. This in its turn is decomposed into calcium hydroxy-hydrosulphide, $Ca(SH)OH$, which is precipitated; thus leaving the water to dissolve another portion of the calcium hydrate.

Secondly, if dry hydrogen sulphide is passed over calcium hydrate, calcium monosulphide and water are formed, in accordance with the equation—

$$Ca(OH)_2 + H_2S = CaS + 2H_2O,$$

a change effected readily and completely at temperatures ranging from 80° to 100° C., but more slowly at ordinary temperatures, owing to the formation of caked masses of the monosulphide, which enclose unaltered particles of the unchanged hydrate. As, however, there seems to be some doubt as to the completeness of the change at ordinary temperatures, the following experiment may be quoted on this point, with a sample of oxide, the preparation of which has been described at length in the author's paper in the *Journal of the Chemical Society.*

I.—*Hydration of the Oxide.*

3·0619grm. of calcium oxide gave 4·1112grm. of calcium hydrate.

		Found.		Theory.
Ratio of $\frac{Ca(OH)_2}{CaO}$	..	1·323	..	1·321

II.—*Synthesis of the Sulphide.—Temperature* 15° *C.*

Conditon of Experiment.	Weight of Calcium Compound, 4·000 Grammes.	Difference. Grammes.	Water Collected. Gramme.	Total Gain. Grammes.
After treatment with hydrogen sulphide for six hours, and hydrogen for two hours	5·5396 ..	1·5396 ..	0·0105 ..	1·5301
„ „	5·6528 ..	0·1132 ..	0·0151 ..	1·6781
„ „	5·6866 ..	0·0228 ..	0·0089 ..	1·7311
„ „	5·7011 ..	0.0118 ..	0·0212 ..	1·7701
„ „	5·7512 ..	0·0398 ..	0·0038 ..	1·3130
„ „	5·7474 ..	–·0038 ..	0·0010 ..	1·8138
„ „	5·7326 ..	–·0118 ..	0·0135 ..	1·8131

At this point the experiment was stopped, as the substance seemed to be only dehydrating slowly, as shown by the loss in weight of lime compound being equal to the gain in weight of the drying tube.

The total gain required by theory for the reaction—

$$Ca(OH)_2 + H_2S = CaS\,xH_2O + (2 - x)H_2O$$

is 1·837 grammes, of which 1·8131 grammes were found, or 98·74 per cent.

III.—*Analysis of the Resultant Substance.*

Gramme.		Gramme.	Percentage.		Mean.
0·3854	gave	0·514 $CaSO_4$	=39·22Ca	....	39·22
0·2240	„	0·463 $BaSO_4$	=27·75	S	27·75
0·2008	„	0·382 $BaSO_4$+0·003S	=27·75		
0·2166	„	0·079 water	=32·03	H_2O	32·18
0·1866	„	0·061	=32·31		
					99·15
		Oxygen, by difference			00·85
					100·00

Atomic ratio of calcium to sulphur = 1·13 : 1.

Comparing, then, the results of the analysis and the synthesis, 0·85 per cent. of oxygen is equivalent to 3·9 per cent. of calcium hydrate. Therefore, of the 4 grammes of calcium hydrate taken, 3·844 grammes only entered into the reaction.

Grammes.	Grammes.	Grammes.
3·844 $Ca(OH)_2$ should yield	3·753CaS+	1·902H_2O
0·156 $Ca(OH)_2$ are equal to	0·118CaO+	0·038H_2O
	3·871CaS+CaO	1·940H_2O
The 4·0 grammes of $Ca(OH)_2$ gave	3·831CaS+CaO	1·960H_2O

The error in both cases is slightly more than 1 per cent., probably accounted for by the difficulty of rigidly excluding all traces of air throughout an experiment lasting many hours, and the inevitable formation of small quantities of calcium thiosulphate. However, both the synthetical and analytical results are sufficient to show that 96 to 98 per cent. of the calcium hydrate is converted into the sulphide at ordinary temperatures. In the course of experiment, it was observed that the above reaction could be more rapidly effected in the presence of water in excess of that required for the production of the hydroxide—a result to be explained by the solution of the larger quantity of the hydroxide, and the greater readiness with which the hydrosulphide is temporarily produced.

The sulphur compounds of calcium have often been described as being of a blue or bluish-green colour; but this tint, which gave rise to the technical term "blue billy," is due to impurities of ferric oxide contained in the native lime. Pure calcium monosulphide is perfectly white; but rapidly turns a pale yellow on exposure to air, owing to the formation of calcium thiosulphate. The green-blue tint of the crude foul lime can be successfully imitated by slaking the pure specimens of the oxide with a solution containing 1 part of iron in 500,000 parts of water, and passing hydrogen sulphide over the product.

Thirdly, if hydrogen sulphide is passed into water in which calcium oxide is suspended, calcium hydrosulphide is formed, in accordance with the equation—

$$Ca(OH)_2xAq + 2H_2S = Ca(SH)_2 + 2H_2OxAq.$$

This substance eventually crystallises out in white acicular needles. This change is the more perfect the lower the temperature at which it is effected; in fact, a freezing mixture is required for the crystallisation of the hydrosulphide. But, in any case, the hydrogen sulphide in excess of that required for the monosulphide is readily evolved by passing into the solution an inert gas, such as hydrogen or nitrogen, or even by mere exposure to the air.[**] Under any of these conditions, the sparingly-soluble hydrated calcium sulphide or calcium hydroxy-hydrosulphide is precipitated in accordance with the reaction—

$$Ca(SH)_2 + OH_2 = CaSH,OH + H_2S,$$

a change accompanied by the evolution of heat, for, according to Thomsen,[††] the substitution of an atom of sulphur by an atom of oxygen in the case of compounds of alkalis and alkaline earths corresponds to a heat evolution of 51·3 heat units.

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[*] Schweigger's Journal, xxxiv. p. 12.
[†] *Ibid.* lv. p. 413.
[‡] Jour. Chem. Soc. 1884, p. 278.
[§] Chemical News, xlix. p. 258.
[‖] Jour. Chem. Soc. 1885, p. 478.
[¶] A precisely-similar phenomenon was observed by Thorpe in the course of the conversion of titanium oxide into the sesquisulphide. (See Jour. Chem. Soc. 1885, p. 491.)

[**] Divers, *cf. supra.*
[††] Jour. für Chem. [2], 101, 21.

Reaction between Calcium Sulphide or Hydrate and Carbon Disulphide.

Nearly fifty years ago, Berzelius[*] described the preparation of calcium sulphocarbonate or thiocarbonate by digesting together calcium sulphide, carbon disulphide, and water; but no analyses were given of the resultant substance. More recently D. Walker[†] prepared a basic calcium thiocarbonate—

$$CaCS_3 2Ca(OH)_2,8H_2O$$

by agitating milk of lime with carbon disulphide—a reaction effected, according to Sestini,[‡] by exposure of such a mixture to sunlight. According to Sestini's description, it would appear highly probable that there can be formed a series of such combinations of the hydroxide and thiocarbonate, varying in colour from a pale yellow to a deep red, according to the proportion of carbon disulphide contained therein. Sestini, Gauvy, and Rotondi proposed to decompose such a liquid by potassium carbonate as a cheap method for the preparation of a solution of potassium thiocarbonate, an antidote to the *phylloxera*; but, according to the experiments of Rotondi, [§] a solution of calcium thiocarbonate could not be obtained containing more than 5 per cent. combined carbon disulphide.

Now, it has been observed, both on the manufacturing and on the small scale, that perfectly-dry calcium sulphide does not absorb carbon disulphide. In the course of the writer's experiments, this point has been determined, first, by passing hydrogen, mixed with the vapour of carbon disulphide, over a weighed quantity of pure dry calcium sulphide; no gain in weight was observed, nor alteration in colour, nor diminution of proportion of carbon disulphide present in the gas as indicated by Mr. Vernon Harcourt's colour test. Secondly, some calcium hydrate was slaked, and moistened so as to form a material of consistency similar to that used in gas-works. *Dry* sulphuretted hydrogen then passed in to convert it into sulphide, and determinations were made of the amount of carbon disulphide absorbed (the relation between the volume of the gas and the volume of the purifying material being the same as that in practice) when dry hydrogen mixed with the vapour of carbon disulphide was passed into it. But this sulphide, instead of absorbing all the carbon disulphide present in the gas, absorbed only 60 per cent., and after a time became inactive; but its activity was restored, and increased to its full value, by merely damping it. This result, on the small scale, is precisely analogous to that obtained on the large scale when the lime purifiers are said to "work dry." On moistening the calcium sulphide with a large quantity of water, and passing carbon disulphide into it, it turns rapidly from a white to an orange yellow, and thence to a red colour; a red liquid ultimately draining down from the sulphide. On evaporation of the liquid, a basic calcium thiocarbonate—$Ca(OH)_2CaCS_3 7H_2O$—separates out. But the nature of the reaction between the calcium sulphides and carbon disulphide can best be studied by passing the vapour of the latter substance into a solution of calcium hydrosulphide. At first hydrogen sulphide is given off in large quantities, with precipitation of the calcium hydroxy-hydrosulphide. When this is completed, the absorption of carbon disulphide commences, as evidenced by the liquid becoming nearly solid by the formation of yellow crystals of another basic calcium thiocarbonate—probably—

$$2Ca(OH)_2CaCS_3,10H2O.$$

These in their turn dissolve, to form the above-mentioned red liquid. It results from these observations that neither dry calcium sulphide nor calcium hydrosulphide is an active material for removing carbon disulphide, but calcium hydroxy-hydrosulphide. The absorption of the carbon disulphide is dependent upon the solution in small quantities of the hydroxy-hydrosulphide, the combination of the dissolved portions with carbon disulphide to form a thiocarbonate whose solubility increases with the proportion of carbon disulphide taken up. But whatever the method of conducting this change, the evolution of hydrogen sulphide is concomitant with the absorption of the carbon disulphide, and the reaction expressing these changes can be written probably thus—

1. $2CaSHOH + CS_2 = Ca(OH)_2,CaCS_3 + H_2S.$
2. $3CaSHOH + CS_2 + OH_2 = 2Ca(OH)_2CaCS_3 + 2H_2S.$

This evolution of hydrogen sulphide was rendered evident, in the usual way, by lead acetate paper. These results are in perfect accordance with the experience of gas engineers, who have noticed that the material from the lime boxes worked according to Mr. H. E. Jones's process (described in his paper read before the Institution of Civil Engineers[‖]) differs entirely from ordinary spent foul lime in emitting a faint odour of carbon disulphide, but no quantity of hydrogen sulphide, and in being slowly oxidisable.[¶] The above experiments explain the reason of the inactivity of a lime purifier, when it has become "over-fouled," or fouled at a low temperature,[**] for the active calcium hydroxy-hydrosulphide has become converted by the excess of hydrogen sulphide into the inactive hydrosulphide. To restore its activity, either air is blown into the purifiers, or the foul lime is taken out, broken up, and returned to the box; the exposure causing it to steam and emit a considerable quantity of hydrogen sulphide. The colour also changes from a white to a dark green; owing, in some way or another, to the conditions being favourable to the formation of the ferrous sulphide. In the former of these cases the blast of air removes the excess of hydrogen sulphide, and causes the production of the active hydroxy-hydrosulphide; while in the latter this change is effected both by the mechanical process of breaking up, and thus exposing unaltered lumps of the hydrate to the action of the emitted hydrogen sulphide, and by the chemical reaction induced by exposure to the air.

PART II.

In order to arrive at an experimental demonstration of the various phenomena experienced by gas engineers in the course of the purification of coal gas from carbon disulphide, experiments were conducted in miniature, but to scale, as regards the relations between the rate of passage of the gas to be purified to the volume of the purifying material. The plan adopted was an imitation of the system of purification in use at the Commercial Gas-works, under Mr. H. E. Jones, which was described at length in his communication to the Institution of Civil Engineers already referred to. Instead of coal gas freed from hydrogen sulphide and carbonic anhydride, there was substituted hydrogen containing a quantity of sulphur, in the form of vapour of carbon disulphide, varying from 10 to 60 grains per 100 cubic feet; the average quantity of "sulphur," as organic sulphur compounds, in coal gas, after leaving the scrubbers, being 30 to 40 grains. According to calculations of Mr. Vernon Harcourt, from results communicated to him by several of the managers of London Gasworks, for every 1000 cubic feet of gas to be purified per hour, 20·3 to 110 cubic feet of lime are used. These figures were taken as the basis of calculation; and on a glass tower was measured off a volume of 0·01 cubic foot. This was filled with pure calcium hydrate of such consistency that it could be slightly moulded with the fingers. The rate of the gas to be purified was varied, according to the condition of the experiment, from 0·5 to 0·091 cubic foot per hour; the maximum rate corresponding to the minimum quantity of lime used per 1000 cubic feet per hour, and conversely.

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[*] Pogg. Ann. vi. p. 411.

[†] Chemical News, xxx. p. 28.

[‡] Gazzetta, i. p. 173; xii. p. 177.

[§] Le Stazioni sperimentali Agrarie (Firenze), 1880, p. 101.

[‖] Proc. Inst. C.E. lxi. Part II.

[¶] *Leicester Greville, vide Journal of Gas Lighting*, 1883, p. 1501.

[**] This was particularly observed by Mr. H. E. Jones, engineer of the Commercial Gas Company, at their Wapping works, in the winter of 1879-80.

The experimental plant was arranged as follows:—First, a Kipp's apparatus for the constant generation of hydrogen, which was freed from any spray of sulphuric acid by passing through bulbs containing water. The gas then flowed over a bulb-shaped tube filled with carbon disulphide; the neck of the tube being drawn out, and provided with a small pin-hole for the vaporisation of the liquid. The gas could then pass in one of two directions—viz., either directly into a glass tower filled with ferric oxide, or through the lime purifier, and thence through the ferric oxide. The object of the oxide was to absorb the hydrogen sulphide either as formed in small quantities by the action of the dilute sulphuric acid on commercial zinc, or as emitted from the purifier. The gas then passed into the colour-test apparatus of Mr. Vernon Harcourt.

Preliminary experiments were made to determine (1) whether the proportion of carbon disulphide vaporised by the hydrogen remained constant, under constant conditions of temperature, within the limits of time in which testings were made; and (2) to ascertain whether the proportion of sulphur indicated by the colour tests varied with the rate at which the gas was drawn through the test apparatus by the aspirator—the rate of passage of gas being estimated by the time occupied by the testing and the volume of water which had run out into the measuring cylinder. The results were found to be satisfactory. Before each set of experiments, blank testings were made; the bulb containing the carbon disulphide being temporarily removed, to ensure that the gas was free from all sulphur compounds. All the testings were conducted in duplicate. In the earlier experiments one-third of the calcium hydrate was converted into the sulphide. The material thus obtained was of a pale yellow colour, due to a slight oxidation. But as this gave unsatisfactory results as regards the removal of the carbon disulphide, another third was converted into sulphide, and the results were satisfactory. The following may be cited:—

SERIES I.—*Rate of Gas (approximately), 5 Cubic Feet per Hour.*

Composition of Gas.	Grains of Sulphur. per 100 Cub. Ft.	Percentage of Sulphur removed.
(1) Sulphur as carbon disulphide present in gas	62	—
(2) After passage through calcium sulphide	4·1	91.5

SERIES II.—*(Conditions as above.)*

(1) Sulphur as carbon disulphide	56	—
(2) After passage through calcium sulphide	nil	100

SERIES III.—*(Conditions as above.)*

(1) Sulphur as carbon disulphide	21·7	—
(2) After passage through calcium sulphide	nil	100

SERIES IV.—*Rate of Gas 0·07 Cubic Foot per Hour.*

(1) Sulphur as carbon disulphide present in gas	80	—
(2) After passing through calcium sulphide	8·1	89·8

These results show that damp calcium sulphide is a fairly-efficient material for removing carbon disulphide. Similar series of experiments were conducted with a sample which had been "sulphided" by dry hydrogen sulphide (*vide supra*); but this absorbed not more than 66 per cent. of the carbon disulphide.

SERIES V.—*Rate of Gas, 0·5 Cubic Foot per Hour.*

Composition of Gas.	Grains of Sulphur per 100 Cub. Ft.	Percentage of Carbon Disulphide removed.
(1) Sulphur as carbon disulphide present in gas	25·7	—
(2) After passing through calcium sulphide	8·6	66·1

SERIES VI.—*(Conditions as above.)*

(1) Sulphur as carbon disulphide	35·7	—
(2) After passing through sulphide	11·1	61·0

SERIES VII.—*(Conditions as above.)*

(1) Sulphur as carbon disulphide	26·3	—
(2) After passing through sulphide	9·1	61·0

Effect of Temperature.

In order to study the effect of temperature, the cylinder containing the sulphide was surrounded by a freezing mixture of ice and salt; and the activity of the material was decreased, as shown by the following results:—

SERIES VIII.—(*At* 15° C.)

Composition of Gas.	Grains of Sulphur per 100 Cub. Ft.	Percentage of Carbon Disulphide removed.
(1) Sulphur as carbon disulphide	20·8	—
(2) After passing through sulphide	7·2	71·1

SERIES IX.—(*At* −10° C.)

(1) Sulphur as carbon disulphide	16·1	—
(2) After passing through sulphide	8·5	50·0

This result explains in a further way the inactivity of purifiers in very cold weather. In this, as in all other observed cases, the rate of chemical change decreases with decrease of temperature.

But in all these cases the yellow colour gradually mounted up the cylinder; thus indicating the evolution of hydrogen sulphide, noticed in Part I.

Effect of Various Gases.

A series of experiments was made with the view of investigating the chemical changes induced by the passage of gases likely to be present in coal gas issuing from the scrubbers, on a sample of foul lime which had absorbed a considerable quantity of carbon disulphide, and assumed a uniform orange-red tint, characteristic of the basic calcium thiocarbonate.

SERIES X.—*Hydrogen.*

Rate of Passage of Gas (approximately), 0·5 *Cubic Foot per Hour. Temperature,* 15° C.

(1) Hydrogen passing through ferric oxide, and thence over platinised pumice	The lead syrup remained unaltered.
(2) Hydrogen passing through the thiocarbonate, and thence over platinised pumice	" "

The calcium thiocarbonate is not decomposed by hydrogen at ordinary temperatures. Similar results were obtained with purified marsh gas.

Hydrogen Sulphide.—To study the effect of hydrogen sulphide, a Wolff's bottle with three tubulures was introduced into the apparatus, immediately after the washing-bulbs. This contained a dilute solution of hydrogen sulphide, the strength of which could be renewed from time to time by passing a few bubbles of the gas by means of the middle tubulure from a Kipp's apparatus. The hydrogen blowing over the surface of the solution carried forward a small quantity of the gas, which was passed immediately into the colour-test, and estimated in the usual way. The experiment was repeated, the hydrogen sulphide being absorbed by the ferric oxide purifier.

SERIES XI.—*Hydrogen Sulphide.*

Rate of Passage of Gas (approximately), 0·5 *Cubic Foot per Hour. Temperature,* 15° C.

Conditions.	Grains of Sulphur per 100 Cub. Ft.
(1) Sulphur present as hydrogen sulphide; the platinised pumice being cold	100
(2) Do. after passage through the "sulphided" lime and ferric oxide	Nil
(3) Same as (2); the platinised pumice being heated:	10·2

From the above experiment it is seen that, whereas all the sulphur as hydrogen sulphide is removed by the sulphide of lime and ferric oxide, there is driven forward a small quantity of carbon disulphide, as indicated by its conversion into hydrogen sulphide on warming the bulb of the colour-test. The result is due probably to a local formation of calcium hydrosulphide and the decomposition of the thiocarbonate. It is in perfect accordance with the observations of gas engineers, who have noticed that, as the quantity of hydrogen sulphide passing into the calcium sulphide purifiers was greater or less, the efficiency of the purifiers was inversely diminished or increased.*

To add confirmatory evidence, a series of experiments was conducted in succession. It was ascertained once for all that no carbon disulphide was present in the apparatus, and that all the hydrogen sulphide contained in the gas was perfectly absorbed by the sulphided lime and ferric oxide.

SERIES XII.

	Grains of Sulphur as Hydrogen Sulphide per 100 Cub. Ft.			Grains of Sulphur as Carbon Disulphide per 100 Cub. Ft., evolved by the Decomposition of the Thiocarbonate.
(1)	167	....	(1*a*)	33·3
(2)	77	....	(2*a*)	22·7
(3)	89	....	(3*a*)	23·8
(4)	79	....	(4*a*)	24·1

From these experiments it follows that the greater the quantity of sulphuretted hydrogen passing into the purifier, the greater the quantity of carbon disulphide eliminated. In fact, these results are precisely analogous to the well-known "gushes or discharges of sulphur" when the purifiers are worked on the Paterson or continuous system.

Effect of Carbonic Anhydride.

It has long been known that before the coal gas passes into the vessels, used for the removal of carbon disulphide, all the carbonic anhydride must be removed, inasmuch as this substance decomposes both calcium sulphide and thiocarbonate, with elimination of hydrogen sulphide and carbon disulphide respectively.† In order to ascertain the magnitude of these decompositions, under conditions likely to occur, the effect of hydrogen containing a quantity of carbonic anhydride equal to that present in coal gas issuing from the scrubbers (500 to 1100 grains per 100 cubic feet) was studied. The artificial gas was prepared by passing into a gasholder filled with hydrogen a few bubbles of carbonic anhydride, the proportion of which was determined by Pettenkoffer's well-known baryta-water method. The mixture, after passing through the washing bulb, entered the calcium sulphide purifier, and thence either directly into the colour test, or through the ferric oxide purifier, and then to the colour test. If the platinised pumice be heated, and the gas passed through the ferric oxide, the amount of carbon disulphide eliminated is determined; but if the gas is not passed through the ferric oxide the amounts of hydrogen sulphide and carbon disulphide together are estimated. The difference between the former and the latter value gives the quantity of hydrogen sulphide. Two conditions are likely to occur in practice. In the one the foul lime purifier may contain unaltered calcium hydrate; in the other, the lime may be completely fouled. The effect of the carbonic anhydride under each of these conditions was observed. The results showed that in the former case carbon disulphide only is eliminated; the unaltered portions of the calcium hydrate absorbing all the hydrogen sulphide, and simultaneously a portion of the carbon disulphide.

SERIES XIII.

Lime Purifier containing Three Parts of Sulphide and Thio-carbonate, and One Part of Unaltered Calcium Hydrate. Rate of Gas (approximately), 0·5 *Cubic Foot per hour; Temperature,* 15° C.

Grains of carbonic anhydride per 100 cubic feet, 1126.

Conditions.	Grains of Sulphur as Carbon Disulphide per 100 Cubic Feet.
(1) Gas passing through sulphided lime and ferric oxide	3·15
(2) Gas passing through sulphided lime only	3·78

SERIES XIV.—(*Conditions as above.*)

Grains of carbonic anhydride per 100 cubic feet, 1183.

	Grains of Sulphur as Carbon Disulphide.
(1) Gas passing through sulphided lime and ferric oxide...........................	2·3
(2) Gas passing through sulphided lime only	2·2

The remaining one-fourth of the calcium hydrate was then converted into sulphide by hydrogen sulphide, the excess of which was blown out by a current of hydrogen before testings were made.

SERIES XV.—(*Conditions as above.*)

Grains of carbonic anhydride per 100 cubic feet = 351.

	Grains per 100 Cubic Feet.
(1) Sulphur as carbon disulphide and hydrogen sulphide liberated............	22·7
(2) Sulphur as carbon disulphide..........	3·0
Difference = hydrogen sulphide ..	19·7

SERIES XVI.—(*Conditions as above.*)

Grains of carbonic anhydride per 100 cubic feet = 117.

(1) Sulphur as carbon disulphide and hydrogen sulphide liberated............	27·0
(2) Sulphur as carbon disulphide..........	3·9
Difference = hydrogen sulphide ..	23·1

SERIES XVII.

Grains of carbonic anhydride per 100 cubic feet = 780

(1) Sulphur as carbon disulphide and hydrogen sulphide liberated	31·7
(2) Sulphur as carbon disulphide..........	6·0
Difference = hydrogen sulphide ..	25·7

Then, under these conditions, not only is hydrogen sulphide emitted, but the proportion of carbon disulphide emitted is increased.

In conclusion the author wishes to record his best thanks to Mr. Vernon Harcourt for kind assistance and advice. He is also greatly indebted to Mr. Jones's paper for the suggestion of some of the experiments above detailed.

[The author illustrated, by experiments, the actual method employed for obtaining the results described in Part II. of the paper.]

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* H. E. Jones, *loc. cit.*

† King's "Treatise on Coal Gas," vol. II. Part iv. p. 22.

DISCUSSION.

The Chairman (Mr. D. Howard) said Mr. Veley's communication was a very interesting one; and the subject of gas purification was one with which the members were all in some way concerned. There were few points more important than the total removal of carbon bisulphide from gas; and he was old enough to remember when it was regarded as almost a hopeless task. The singularly-easy method whereby it was at present removed, the chemistry of which had been so ably explained, was one of those happy adaptations of a very simple process which was really amongst the most valuable discoveries in chemistry.

Professor Foster said there was one point in connection with this question, of the action of sulphuretted hydrogen on lime, which he did not think had yet been quite cleared up. Dr. Divers, in his admirable paper, gave a considerable amount of information with reference to the hydrosulphide and the hydroxy-hydrosulphide described by Mr. Veley.* At the time he took occasion to note that if, by his statement, Dr. Divers meant that the treatment of calcium hydrate with sulphuretted hydrogen was attended with the production of hydrosulphide only, under the conditions which obtained in the ordinary lime purifier of the gas engineer, he could not agree with him. Like most men engaged in matters relating to coal gas, he had worked on this lime question; but all alike seemed to be in the dark until Dr. Divers's paper appeared. The subject of his own inquiry had then resolved itself into the action of sulphuretted hydrogen on pure lime, although he began with a large number of specimens of spent lime; the results being very similar to those of his predecessors—viz., entirely unsatisfactory. Dealing with commercial lime, he endeavoured to get at some knowledge of what happened when sulphuretted hydrogen was passed into it, commencing at the ordinary temperature, and keeping well within the temperatures which were produced by the chemical action of the sulphuretted hydrogen on the calcium hydrate. The result of his inquiry was to lead to the following conclusion:—That if calcium hydrate was treated with sulphuretted hydrogen in this way, under the ordinary conditions, this result (expressed empirically) was obtained—$CaOCaS2H_2O$. Expressed constitutionally (following Dr. Divers), the formula was—$Ca2HOCaHOHS$. The point was this—that such a compound only contained one-half the sulphur which Dr. Divers and Mr. Veley attributed to the compound in question. Dr. Divers stated in his paper that calcium hydrosulphide was produced in the lime purifiers. It might be that some of the conditions could be explained, because they all knew, from the experience of Dr. Divers and Mr. Veley, that hydrosulphide underwent splitting up into calcium hydroxy-hydrosulphide, with a production of calcium hydrate. What he wished to point out was (and it was a practical matter for gas managers) that, in the purifiers charged with calcium hydrate, the condition of things named would result. Speaking from memory, the compound contained 20 per cent. of sulphur. Another practical point was this: He had never met with a specimen of spent lime obtained in the ordinary way, and containing moisture, in a condition which had more than 10 per cent. of sulphur. Therefore, if a gas engineer followed the formula as he had given it—and he believed it to be quite possible—if he tried to realise this condition of things, he would never ordinarily reach more than 20 per cent.; and however long he tried he would never succeed in getting much beyond this. The action was a perfectly definite one. If a tube containing calcium hydrate were taken, and if sulphuretted hydrogen were then passed—a dryer and an absorber of oxygen being arranged intermediately—the action was quite progressive. Hour after hour the progression of the sulphuretted hydrogen could be seen, and as long as there was a particle of lime in front no sulphuretted hydrogen passed through the tube. Sulphuretted hydrogen might even be passed through for some time, and after it escaped freely, if the compound were then removed and dried over sulphuric acid such figures as these would eventually be found—$CaOCaS2H_2O$. He believed that Dr. Divers's statement and his own experience might be reconciled. All he contended was that, in the lime purifier, as ordinarily charged, the formula which he had given was nearly the actual condition of things that would obtain under such circumstances. If, therefore, one used the constitutional formula—showing a so-called molecular compound—they could understand how the action on bisulphide of carbon took place. In relation to the paper, many other questions arose which he would have liked to go into; but, after all, this was the practical point—that one could not get anything like the percentage of sulphur into the calcium hydrate which the able experiments of Dr. Divers had incidentally led people to believe. It might possibly be done at a very low temperature, but unless the gas engineer realised such conditions he would fail in his task. From a money point of view, it meant, of course, that if calcium hydrate were used for the purpose of taking out sulphur, there again arose the question of expense in effecting the object, because he could in this way never get more than such a ratio, unless he passed sulphuretted hydrogen for a very prolonged time. He had specimens of lime obtained by passing sulphuretted hydrogen, taking out the mixture and allowing it to be again treated; but he never obtained, under these conditions, with ordinary dry hydrate, 1 atom of calcium to 1 of sulphur.

Mr. C. W. Folkard said it had been remarked by the last speaker that, in the ordinary process of purification in gas-works, it was impossible to get more than about 50 per cent. of the lime transformed into sulphide. Two years ago he published in the *Chemical News*, the results of some experiments he had conducted on a small scale, which showed very plainly that this was the case; and he frequently analysed the material coming from the lime purifiers, and had never been able to get more than 51 per cent. transformed—it varied from about 42 to 51 per cent.—even after the gas containing sulphuretted hydrogen had been passed until a large quantity had come right through the purifier for several days. He thought it exceedingly probable that this was due to a matter which had been referred to earlier in the evening—viz., to the action of mass—because the crude gas contained an exceedingly small proportion of sulphuretted hydrogen. He could understand that, if pure sulphuretted hydrogen were passed into slaked lime, it was quite possible that the reaction would be such as to transform the whole of the lime into calcium sulphide, instead of getting a molecule of calcium sulphide to combine with one of unaltered lime, which was undoubtedly the form in which the reaction took place in ordinary gas-works. He would ask Mr. Veley to tell them exactly how he prepared the sample of slaked lime. He had been careful to proportion the amount of gas passing through to the bulk of material. But there was another small point which made a great deal of difference in these matters, and it was this—that the depth of material was not the same. In an ordinary purifier the material would be something like 24 inches deep, and therefore the gas would pass very much more rapidly past a given particle of lime. Mr. Veley had properly proportioned the cubic amount of purifying material to the volume of gas, but had not the same surface area as existed in gas-works. He (Mr. Folkard) should think that it would be about one-eighth or one-tenth, and this would make a vast difference in the result. It was well known that if the purifiers were three or four times the usual area, the purifying action was much greater, whereas Mr. Veley's experimental purifier had about one-tenth the area usual in gas-works. It had been found, in experimenting on a small scale, that almost anything would take out carbon disulphide, therefore these small experiments were always liable to lead to error.

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* See Journal of Gas Lighting, Sept. 9, 1884 p. 471.

Dr. P. F. Frankland asked Mr. Veley whether he had the proportionate amount of sulphur compounds in the gas as well as the total quantity of gas. If this were a question of mass action, as Mr. Folkard suggested, it was, he said, obvious that very different results would be obtained according as a larger or a smaller proportion of sulphide of carbon was used respectively to a smaller or larger proportion of gas.

Mr. Veley, in reply, said he had conducted the experiments in this way: Starting with a known quantity of lime, he passed the sulphuretted hydrogen into it, and weighed the material, and also the water which was formed. Making a synthesis of the product, he had shown that, starting with a known weight of lime, and passing in sulphuretted hydrogen until all the change was complete, as shown by the fact that the continuation of the passage of the gas merely caused a dehydration of the material (that was to say, the loss of the tube containing the lime was exactly equal to the gain found in the water collected) the result was—

$$Ca(OH)_2 + H_2S = CaSH_xO + (2-x)H_2O.$$

Making analyses of this material, he found the result coincided almost exactly with that of his synthesis. Even according to Professor Foster's own admission, substances examined by him were really admixtures of calcium sulphide and unaltered lime; for he himself said that if he took out the lime, shook it about and otherwise disturbed it, put it back again, and then repeated the action with sulphuretted hydrogen, he could get a larger quantity of the gas absorbed.

Professor Foster said this was scarcely so. What he wanted to point out was that, when sulphuretted hydrogen was passed into slaked lime at the ordinary temperature of a room, the affinity or attraction was most marked, and the sulphuretted hydrogen progressed most slowly, so that not a particle of lime escaped its action; then the action stopped, and the sulphuretted hydrogen passed most freely. If the lime were taken out and rubbed up, and then had a dose of sulphuretted hydrogen given to it, the percentage might be slightly increased. Whether the increase in the action of the lime was due to oxidation, to the rubbing up, or to the hydrolysis through the presence of moisture, he could not say; but the action, in the first instance, was a definite one, and seemed to stop at a certain stage.

Mr. Veley said he had never arrived at this stage, but always found it go on to a further one. If the experiment at this stage were carried on at the ordinary temperature for many days, then the result was complete. He took it that Professor Foster's results were due to what he (Mr. Veley) had always found an exceedingly practical difficulty—damp calcium sulphide caking round, and thus enclosing nuclei of unaltered lime. It was obvious that if a certain amount of calcium sulphide caked together, as it would, there would be also included in such a caked mass quantities of unaltered lime. The great practical difficulty in working was to avoid this caking together of material. Mr. Folkard had inquired how he prepared his slaked lime. For these experiments he started with a known quantity of purified calcium oxide, from which all impurities (such as iron, silica, barium, and strontium) were removed. He hydrated it in an apparatus devised to eliminate all traces of carbon anhydride, and then succeeded in converting a certain weight of calcium oxide into the theoretical quantity of calcium hydrate (the figures were given in the paper). This material was put into a tube, and perfectly dry sulphuretted hydrogen, from which all traces of oxygen had been carefully removed, sent in. It did not follow that, when some sulphuretted hydrogen passed over, the reaction was therefore complete; in fact, the reaction would go on slightly even, for a long time after volumes of sulphuretted hydrogen passed through the lime purifier, owing, as he took it, to the caking together of these damp masses of calcium sulphide. For these experiments pure lime was mixed with water so as to form a substance of similar consistency to that used by gas engineers. He had made many experiments, varying the rate and the proportion of carbon disulphide, and under these conditions all the carbon disulphide had been removed. If, then, it was all removed, taking the minimum volume of lime with the maximum quantity of gas containing the largest quantity of carbon disulphide likely to be present in practice—*a fortiori* this would be removed when a lower rate was taken, and when the gas contained a smaller proportion of carbon disulphide. He could not agreed with the suggestion of Mr. Folkard that it was proper to take the relation of the area of the purifying material to the volume of gas to be purified. It appeared to him that in this case two different kinds of quantities were being dealt with—square feet of purifying material and cubic feet of gas to be purified, and this seemed to be rather an irrational way of looking at it. They must be put in the same terms—viz., volume of purifying material to volume of gas; for, even although the gas to be purified passed over, it also passed through the cubic contents of the purifier. Thus he thought the proper relation to be taken was that of volume to volume. When such a purifier had been in action for some time all the sulphuretted hydrogen was gradually driven off, and the lime from this purifier merely possessed the odour of carbon disulphide, not the peculiar disagreeable odour of ordinary foul lime.

Liverpool Section.

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Vice-Chairman: Prof. J. Campbell Brown.
Committee:

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John Hargreaves.
E. Milner.
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H. Brunner.

A. Norman Tate.
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University College, Ashton Street.

THE PRESIDENT IN THE CHAIR.

THE COMPARATIVE EFFICIENCY OF VARIOUS MODES OF TREATING LIQUIDS WITH GASES.

F. HURTER, PH.D.

When our engineers are asked to erect any structure, they calculate the probable weight which it has to carry, or the stresses it is subject to; they take out of their pocket a valuable thing called a "Molesworth," or some other book filled with formulæ, and by means of them calculate the dimensions of their structure. Their skill and experience is employed in choosing and combining the materials and the factors of safety, and they trust the rest to Molesworth and providence. I am told that most of the structures of the chemical works in Widnes are based upon these formulæ collected in Molesworth. Why can we trust so implicitly to such formulæ? Simply because they are the most concise record of the experiences of our most eminent engineers; and not only a record of their experience, but also of their success in tracing out the causes of the phenomena they have observed.

In the construction of chemical apparatus, as far as the strength is concerned, the same facility is afforded; but as regards every other particular we are left entirely to our fancy, or in the case of long experience, to that second sight which is gradually and unconsciously acquired. But with the termination of the career of many a successful man terminates also his whole experience. Owing to the fact that his experience was not in a form sufficiently concise to be readily transmitted to others, that it was not formulated experience—it is entirely lost. I have frequently felt the want of such guides to the best form and general construction of new apparatus, as the engineers possess for finding the dimensions, when the general form is given to them. I have consequently always been anxious to collect isolated experiments under a general formula. I have tried to collect the whole of the experience of myself and others of vitriol chambers in such a single formula, and I brought the results of such an attempt before this Society some years ago.

To-night I wish to discuss the comparative efficiency of various modes of treating liquids with gases. I meant to discuss all the modes now in use, but I found the subject too large for one paper. I have decided, therefore, to discuss only one class of apparatus, but so that the paper will form an independent whole, and I may, at some future meeting, if it be your pleasure, discuss the remainder of the subject.

I.

The treatment of a liquid with a gas has usually one or both of two ends in view: either we wish to saturate completely a liquid with one of the constituents of a gaseous mixture, or we want to deprive the gas wholly of one of its components, or we are obliged to accomplish both the saturation of the liquid and the complete absorption of one of the gaseous substances constituting the mixture. Sometimes a liquid is treated with a gas for the purpose of removing from the liquid a volatile compound.

For whichever of these purposes we treat a liquid with a gas, the mechanical part of the problem is always the same: we must bring the gas into intimate contact with the liquid. This intimate contact can only be brought about by the expenditure of energy; we have to do a certain amount of mechanical work. But the problem of treating liquids with gases is not so simple as to resolve itself merely into a discussion of how to produce the intimate contact with the least expenditure of work; we might produce a very intimate contact very cheaply, but afterwards waste the contact by otherwise badly arranging our apparatus—*i.e.*, obtain a very small effect in spite of the intimate contact—if we paid not sufficient attention to the chemical part of the problem.

Before we can speak either of the mechanical efficiency of a piece of apparatus producing the necessary contact, or of the efficient utilisation of the contact so produced, we must have some measure for the contact itself—we must have a unit of contact. The amount of absorption which will take place between a gas and a liquid will depend entirely upon the number of molecules of the gas which will, during a given time, impinge upon the liquid—*i.e.*, it will be proportional to the extent of the border-surface between the liquid and the gas, to the number of molecules which impinge upon the unit area of this surface in unit of time, and to the length of time during which these conditions obtain. As the number of molecules which impinge upon unit-surface in unit of time is proportional to the pressure of the gas, the pressure itself may be substituted for that unknowable number. I shall, therefore, define (for the purposes of my comparison of the efficiency of various apparatus) one unit of contact to mean that unknown number of molecules which would impinge upon unit area in unit of time if the gas exerted unit pressure.

For technical reasons I choose the following units:—As unit area, the square meter; as unit pressure, 1 meter of water; as unit time, 1 second. So that 1 unit of contact means the number of molecules of a gas which would impinge in one second upon 1 square meter area if the gas were under a pressure of one meter of water.

Five units of contact might be obtained from three different sets of conditions; we might have five square meters' area acting during one second with the gas of one meter pressure, or we might refer to one square meter acting for five seconds with a gas of one meter pressure, or we might refer to a surface of one square meter maintained for one second, the gas being under a pressure of five meters of water. In each of these cases the actual number of molecules impinging would be exactly the same, and, consequently, if in all three cases the chemical condition of the liquid and the gas were the same, the rate of absorption would also be the same.

The mechanical efficiency of any piece of apparatus can thus be measured by the number of such units of contact which it yields for a given expenditure of work, say for the expenditure of one kilogrammeter.

All apparatus for bringing into contact liquids and gases can be divided into three classes, according to the method employed for producing the contact-surface.

(1.) The gas, divided into small bubbles, passes through a column of liquid.

(2.) The liquid, divided into fine drops, falls through a column of gas.

(3.) Solids, with extensive surfaces and interstices, are covered with the liquid, the gas filling up the interstices.

A comparison of the efficiency of these various classes of apparatus is very difficult, and I have not completely solved the problem; but what I have done will, I hope, form a foundation for other investigators, and form, to some extent, a guide to the choice of apparatus for any given case.

That such a choice is sometimes extremely difficult, is illustrated by the various apparatus which various technologists prefer for one and the same process. Thus, for instance, I found when I came to this country, just at the time when Mr. Weldon's process for the recovery of manganese was about to be introduced, one chemist attempting to subdivide the liquor in which the manganous oxide was suspended into fine spray, and allow it to fall through air. In a patent (not Mr. Weldon's) I found a proposal to project that liquid into the air by means of paddle-wheels. Then we have Mr. Weldon's own solution of the problem—the best—which consists, as you are well aware, in injecting the air into the liquid.

I shall confine my remarks to-night to apparatus of the first class—that where the gas is divided into fine bubbles, and made to pass through a column of liquid.

This method of treating a liquid with a gas has, at first sight, the great disadvantage, that the mechanical power required for the production of the contact is very great. Thus, for instance, it requires the expenditure of 998 kilogrammeters to propel one cub. meter of gas through a column of water one meter deep; so that the work done in propelling one kilogramme of gas through one meter of liquid is equal to that of lifting one kilogramme of water 1000 meters high. When, in addition to this high expenditure of work per cub. meter of gas, we consider that every cub. meter of liquid has usually to be treated with an enormous bulk of gas varying in different technical processes from 200 to 20,000 cub. meters, we need not be surprised that now and then attempts are made to avoid using this apparently very costly method.

The apparatus used for this mode of treating liquids with gases is usually extremely simple, and it cannot be too simple. In most cases the apparatus consists of a vertical cylindrical vessel holding the liquid. At the bottom of the vessel is usually placed a system of pipes ending in numerous small orifices, so as to spread the gas and distribute it equally over the area of the vessel. The gas is delivered by an air pump, which compresses it to a pressure slightly greater than that due to the depth of the column of liquid through which the gas has to rise. In other cases the vessel is closed at the top, and the space over the liquid is exhausted, so that the gas is drawn through the liquid instead of forced through. In other instances, for example, in the apparatus Mr. Chance used for carbonating the mixture of magnesia and calcium chloride, the vessel is closed at the top, and provided with a loaded valve, so as to enable the liquid to be treated under a much greater pressure than that due to the column of liquid contained in the vessel. In all these cases, the work to be done, and the result obtained, are very different.

One great difficulty presents itself at once when we wish to deal arithmetically with the question of efficiency of this kind of apparatus. It is impossible to measure the surface which the gas bubbles present to the liquid; we can only make some approximate estimation. But it is possible to ascertain how the surface of the gas bubbles varies under different circumstances, such as depth of liquid, nature of the liquid and the gas. I have made this the subject of a few experiments. The first question which I attempted to answer was, How does the size of a gas bubble issuing from a given orifice vary as that orifice is immersed to different depths in the same liquid? I found that the diameter of the bubble was always the same, however great I made the depth of the column. I varied the depth of the column from 0.2 meters to 10 meters, and I found no greater difference than 1 per cent. in the value of the diameter of the bubbles. I repeated the experiments with various orifices, which, of course, gave bubbles of different diameters; but one and the same orifice always gives bubbles of one diameter, whatever the depth of the liquid above the orifice may be. The same result is obtained whether the orifice is simply the end of a straight pipe, or whether it is a pin hole in a sheet of platinum; the bubbles from the pin hole will be smaller, but they are all equal to, and independent of, the column of liquid above. I also found that for a given orifice the size of the bubbles was independent of the nature of the gas. Whether the gas was hydrogen, air, coal gas, or carbonic acid gas, the bubbles from the same orifice were equally large. The only great alteration in the size of the bubbles is produced by a change in the liquid itself. A very small addition of alcohol to water will materially reduce the size of the bubbles formed by a given orifice.

These experiments lead to the conclusion that the size of the bubbles obtained from any gas by passing it through a given distributor is independent of the pressure above the distributor. Of course, as the bubbles rise they expand, and the amount of this expansion will depend upon the height of the column. But at the point at which the bubbles leave the distributor, their diameter will only depend upon the size of the openings of the distributor, and upon the viscosity of the liquid.

The next question which I tried to settle by experiment was, How do the bubbles move upward, is their velocity accelerated, or retarded, or uniform? And, if possible, I wanted to obtain its magnitude.

The velocity of bubbles in a wide glass tube I found so small that, by means of a metronom beating 240 beats a minute, I could obtain some approximate idea of the velocity. I found no appreciable acceleration; the motion was, as far as I could judge, perfectly uniform. I ascertained the velocity to be about 0·3 meters per second, or not quite one foot per second. I felt, however, that the results of so rough an experiment would not form a secure basis for any important calculations. Conclusions from an experiment, where one bubble was made to rise, and only one, through a quiescent liquid, might not apply to a case where thousands of bubbles are on their way through a liquid in violent agitation, as we have it in a Weldon oxidiser. I therefore endeavoured to ascertain the average velocity of such bubbles in a Weldon oxidiser. This, at first sight a difficult problem, is really very easily solved. Owing to the slow motion of bubbles through the liquid, there are a considerable number of them on their way through the liquid at any moment, they displace a certain amount of liquid, consequently the level of the liquid rises. If we measure the amount which the liquid rises, ascertain at the same time the rate at which the gas is pumped through the liquid, also the height of the liquid when quiescent, and the area of the vessel, we have all the data needed for calculating the average velocity of the air bubbles in the Weldon oxidiser. Clearly the amount of liquid displaced gives us the amount of gas in the liquid at any moment, from the distributor to the surface of the liquid. Fancy you followed with your eye the bottom layer of bubbles as they gradually rise; by the time this layer has arrived at the surface, the whole vessel has been supplied with as much gas again as it contained when that layer was at the bottom; therefore, the time which it takes for the air pump to supply that gas is also the time which that layer of bubbles required to rise to the surface.

If D is the depth of the liquid when quiescent, and *d* the amount it rises, when gas is blown through it at the rate of C volumes per second (measured at the average pressure in the liquid) and if A is the area of the vessel, the amount of the liquid displaced will be A.d., the number of seconds which it will take to deliver that amount will be $\frac{A.d}{C}$ and in $\frac{A.d}{C}$ seconds the gas bubbles rise through the total height D + d., consequently their velocity is

$$v = \frac{C(D+d)}{A.d.}$$

I will give the results of one such experiment, which serves as a numerical example. An oxidiser was charged to a depth of 15 feet, it had an area of 100 square feet; air was blown into it at the rate of 14·8 cubic feet (measured at a pressure of 7½ feet of liquid). I found the charge to rise 2·66 feet. Thus the displacement was 266 cubic feet, and the time necessary to deliver this amount 18·3 seconds, this being also the time necessary for the bubbles to pass through a column now 17·66 feet in height. The average velocity is thus $\frac{17·66}{18·3}$ or ·95 feet per second. This shows that the average velocity in such apparatus does not differ materially from that found in the experimental tube. (Before passing on with my subject, I will just draw attention to the fact, that the velocity of bubbles being taken at 1 foot per second approximately, the formula just given may be used for calculating the amount which a liquid will rise in *any vessel when gas is blown through at a certain* rate; the formula for that purpose assumes the form

$$d = \frac{D.C}{\sqrt{A.}-C})$$

Thus my experiments made also with various depths of liquids have shown me that for practical purposes the velocity of these bubbles is nearly constant, and about 0·3 meters per second.

Owing to the fact that the diameter of the bubbles is always the same whatever the pressure is, one cubic meter of gas measured at atmospheric pressure will not always yield the same surface in the same distributor, that depends upon the depth of the column of liquid. For although the bubbles are always of the same diameter, the weight of gas contained in one bubble is of course greater the greater the depth of the column of liquid.

As the bubbles rise through the column of liquid they expand, and thus enlarge their surface. At the same time the pressure diminishes. It is not difficult to calculate the total number of units of contact thus produced for each bubble in rising. If this is multiplied by the number of bubbles which would make up one cub. meter of gas at ordinary barometric pressure, a formula is readily found, which will give the number of units of contact produced per cub. meter of gas.

For the three modifications of apparatus in which bubbles are made to rise through the liquid, these formulæ are the following three:—

(1.) For blowing gas through a column of depth D, the barometric pressure being *b* meters of the same liquid, and the velocity of bubbles 0·3 meters a second *k* the co-efficient of distributor—*i.e.*, number of square meters it produces per cub. meter of gas, when the bubbles issue at atmospheric pressure.

$$\text{(1.) Units of contact per Cub. meter gas......} = \frac{10}{4}\,k : b^{2}\left(1+\frac{D}{b}-\sqrt[3]{\frac{b}{b+D}}\right)$$

(2.) If the gas is pulled through the vessel by a vacuum *pump*, the *symbols having the same* meaning—

$$\text{(2.) Units of contact per Cub. meter gas ..} = \frac{10}{1}\,k.b^{2}\left(1-\left(\frac{b-D}{b}\right)\sqrt[3]{\frac{b-D}{b}}\right)$$

(3.) If the gas is compressed and passed through a column of liquid of depth D, and allowed to escape through a loaded valve maintaining an absolute pressure *p* at the surface of the liquid, the units of contact will be—

$$\text{(3.) Units of contact per Cub. meter gas..} = \frac{10}{1}\,k.b\left(\left(D+p\left(1-\sqrt[3]{\frac{p}{p+D}}\right)\right)\right)$$

Such formulæ in themselves convey to those not accustomed to read them no information whatever. I shall, however, give below a numerical comparison, which will show a striking difference in the cost of the three methods.

I have defined the efficiency of a piece of apparatus as the number of units of contact produced per kilogrammeter of work spent. If the amount of work to be done per cub. meter of air is known, we have only to divide the units of contact by the number of kilogrammeters to obtain the efficiency.

Supposing, in the first instance, we had a vessel holding 4 meters a liquid of sp. gr. 1·03, for which $b=10$, at the bottom of which we had a distributor yielding bubbles of 6mm. diameter which yields per cub. meter 1000 square meters of area, so that $k=1000$. The number of contacts which would be obtained—

1. If the vessel is open at top, and the gas forced through, it would be according to formula (1) 126,525 per cubic meter.

2. If the vessel is closed at top, and the gas drawn through by a vacuum pump, according to formula (2) 123,485 per cubic meter.

3. If the vessel is closed at the top, and the gas escapes through a loaded valve at a pressure of 1 atmosphere = 14·7lb. to the square inch = 29·4lb. absolute or = 20 meters of our *liquid*, we have 129,475 per cubic meter.

These numerical results show that as far as the intimate contact is concerned, the same distributor will produce very nearly the same number of contacts whichever way it is applied. I discovered this fact—viz., that a given distributor produces the same result with the same height of column absolutely, whether the pressure above that liquid be 35lb. to the square inch or *nil* first, on experimenting on the large scale with a column of liquid 10ft. deep, containing caustic alkali which had to be carbonated. The percentage of carbonic acid absorbed is independent of the pressure of the carbonic acid gas, whichever of the three modes of applying gas to the liquid is chosen. But when we come to consider the power employed to produce this contact, we find it for the three cases to be:—

1. For simply blowing the gas through four meters' depth of liquid per cubic meter of gas, 3643 kilogrammeters.

2. For simply pulling the gas through four meters of liquid, 4894 kilogrammeters.

3. For keeping the liquid under a pressure of one atmosphere . . . 10,274 kilogrammeters.

The efficiency of the apparatus is therefore:—

No. 1.......... 31·73 units per kilogrammeter.
No. 2.......... 25·23 „
No. 3.......... 12·60 „

These results show that the method of simply blowing air through a liquid is by far the cheapest of the three. Theoretically, the cost decreases as the column of liquid is increased, for methods one and three. Thus for 10 meters' depth of liquid, the efficiency of Nos. 1 and 3 become—

No. 1.. 38·12 units of contact per kilogrammeter.
No. 3.. 23·19 „ „

but practically the efficiency becomes less and less, owing to the difficulties which present themselves when gases have to be pumped against very high pressures.

The efficiency of the second modification diminishes very rapidly as the column is increased in height. That method is always to be avoided whenever it can be avoided. It is usually only employed where corrosive gases have to be absorbed, so as to avoid contact between the propeller and the corrosive gas.

There is one great drawback in all three modes: it is difficult to have a perfect counter-current, which for most chemical reactions is of very great advantage. By counter-current, I mean two streams moving in opposite directions, so that the saturated liquid leaves the apparatus where the *fresh* gas enters; whereas, the exhausted gas leaves the apparatus where the fresh liquid enters. This cannot be arranged to perfection with the method of gas bubbles, but in every other respect that method is little inferior to any.

DISCUSSION.

In answer to questions, Dr. Hurter said that the pressures mentioned in his paper were absolute, and included barometric pressure; thus, he would call an absolute pressure of gas 4, which was capable of overcoming a column of water 4 meters in height.

Dr. Campbell Brown, while admitting that the paper was a model of philosophic argument and close reasoning, felt that the results arrived at were certainly unexpected. He would like to know whether air and water only, or whether soluble gases, such as chlorine, had been used; as the latter would complicate the results, but at the same time render them more valuable.

The Chairman asked how the force was measured in the case of a vacuum.

Mr. Thompson asked whether the size of the bubbles bore any ratio to that of the apertures? Also, supposing that an excess of force above that necessary to overcome the column of liquid were used, would the velocity of the bubbles be materially increased? In making some silicofluoric acid a short time since he had found that when he inserted his delivery pipe under mercury covered by water, the bubbles, which at first were large, diminished rapidly in size, until at last, when the gas became pure, they were reduced to very small dimensions.

Dr. Hurter, in answer to the Chairman, said that his figures were theoretical; in practice this efficiency was never realised, and the force required vastly exceeded that deduced from calculation. In answer to Mr. Thompson's question as to the size of the bubbles, he had not as yet satisfactorily determined their relation to that of the apertures. As regarded velocity, force above that required to overcome the pressure of the column was useless, as friction soon reduced any extra velocity so obtained to that of ordinary floatation; as regarded silicofluoric acid, as the gas increased in purity the bubbles diminished, until, with pure gas, they saw no bubbles at all.

Dr. Hewitt said that the late Dr. Angus Smith had been in favour of absorbing hydrochloric acid by means of a spray, and had shown him a design of apparatus to effect absorption by such means. Dr. Hurter evidently dissented from this notion. If, then, some definite law could be established, these questions would cease to become mere matters of opinion.

The Chairman said that the meeting were much indebted to the author for his valuable communication. He must confess his surprise at the figures given for the disproportion between the amount of force expended when gas was passed through a liquid in the one case, and presented in the form of spray in the other. He had always been in favour of the former method, but had no idea that the difference in cost was so great. Corrosive gases, however, required special methods of absorption. Many patents had been taken out for absorption in the form of spray, but it was well that manufacturers should be made aware that such methods involved great cost.

Manchester Section.

Chairman: Sir H. E. Roscoe.
Vice-Chairman: I. Levinstein.
Committee:
R. F. Carpenter.
C. Estcourt.
H. Grimshaw.
B. W. Gerland.
Peter Hart.
T. Jackson.
D. B. Hewitt.
C. Schorlemmer.
Watson Smith.
L. Siebold.
Wm. Thomson.
D. Watson.
Local Secretary:
J. Carter-Bell, Bankfield, The Cliff, Higher Broughton, Manchester.

Meetings, Session 1885—86.—First Tuesday in each Month, at 7 P.M.

December 1.—A. H. Allen—"Further Notes on the Chemistry and Analytical Examination of Fixed Oils."

Watson Smith—"The Violet Stains appearing on Sized Cotton Cloth" (*Experimentally Illustrated*).

Notices of papers and communications for the meetings to be sent to the Local Secretary.

The opening meeting of the session was held in the Chemical Club Rooms at the Victoria Hotel, on Tuesday evening, 3rd November. The Chairman (Sir Henry E. Roscoe) in the chair.

The Chairman, after congratulating the members on the convenient situation of their meeting room, and on the active and promising condition of their Section, proceeded to point out the success achieved by members of the Society at the International Inventions Exhibition. The award of gold and silver medals in such large numbers to members of our Society was a very satisfactory state of things, as it betokened activity and showed that in the essential conditions of originality and of practical power the industrial chemists of England were not behind those of other countries. He then alluded to the great loss which the Society had sustained in the lamented death of their late President, Mr. Walter Weldon, and paid a well-deserved tribute of affectionate respect to the memory of one who had raised high the standard of science in its applications to the most important of our national chemical industries, and whose name would for ever be honourably associated with those industries, and with the Society which he helped to establish and did so much to adorn.

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NOTE ON ANTIMONY COMPOUNDS USED IN DYEING AND PRINTING.

BY B. W. GERLAND, PH.D.

The use of aniline colours in cotton dyeing and printing was largely extended by the introduction, through Messrs. Brooks & Co., of this city, of tartar emetic for fixing baths. The efficient constituent of the tartar emetic is antimonious oxide, which forms an insoluble lake of great resistance with the colour-tannate. The other constituent, bitartrate of potass (a rather costly substance, and likely to become dearer in consequence of the spread of the vine louse and increased demand), only serves to keep the metallic oxide in solution, and is lost with the waste waters. As the metallic oxide only amounts to 43·7 per cent. of the salt, its price in this form is very much enhanced. At the time, tartar emetic was the only compound of antimony in the market possessing the required qualities, and efforts were now made to discover one combining cheapness with these properties. As the outcome of these we have now two substitutes that are largely sold. One is the fluoride of antimony proposed by Mr. S. Mellor, of the Magnesium Metal Company, and manufactured in three forms by this firm—the acid and the neutral liquids, and the third as a white crystalline powder. The latter dissolves readily in boiling water, and like the others, can be diluted with water without separation of antimonious oxide. I am informed that of all antimony preparations these are the cheapest, and give satisfaction in the absence of light colours and ultramarine, which is discharged even by their dilute solution. I was anxious to experiment upon these fluorides, but the samples kindly sent by the Company only arrived yesterday. The other substitute for tartar emetic is the double salt of antimonious and potassic oxalate, supplied largely by Messrs.

Rudolph Koepp & Co., of Oestrich on the Rhine. They submitted this preparation to the judgment of Dr. Noelting, Professor of the School of Chemistry in Mühlhausen, and received a very favourable report. According to this, the double oxalate did not only do the work as efficiently and as well as the tartar emetic, but it could, moreover, replace the latter weight for weight, although containing only a little over one-half the quantity of antimony. Dr. George Hirzel, of the Augsburg Printworks, instituted comparative trials between the oxalate and tartrate, and arrived at results differing widely from Noelting's (*Chem. Zeit.* No. 41, page 325). Noelting's reply soon followed, and as this enters fully upon all points, and gives also the opinions of Mühlhausen printers, I cannot do better than lay a full extract of the same before you.

Hirzel mentioned trials on a large scale with printed pieces, which were fixed in baths containing respectively 10grm. tartar emetic, 10grm. antimony oxalate, and a third containing 19grm. of the latter per litre. The quantity of antimony in this bath was equal to that of the 10grm. tartar emetic bath. The pieces taken through the tartar emetic bath showed the fullest and brightest colours; those fixed in the weaker oxalate bath were weaker in colour, and even the colours of those which had been through the stronger oxalate solution were not equal in depth to the first lot. Similar results were obtained in a neighbouring printworks, and there a want of brilliancy in the colours was noted in the pieces fixed with the oxalate in comparison to those fixed in the tartrate. No better results were obtained by adding chalk, soda, or other neutralising agents to the fixing baths. Noelting, after referring to the results obtained in his laboratory, states that these have been completely verified in the Mühlhausen Printworks, where the antimony oxalate is now used regularly, and explains the greater efficacy of the antimonious oxide in the oxalate by the greater inclination to dissociation of the latter. The fixation of the tannin is consequently quicker and more complete, but the baths are sooner exhausted, which, however, is an advantage, since the tartar emetic baths have oftener to be renewed before exhaustion, in consequence of having become fouled by colours stripped from the prints. The oxalate, according to the experience of Mühlhausen dyers, can also be used for uni-dying, where the cloth mordanted with tannin is taken through the antimony bath and then dyed. In this case the oxalate also replaces an equal part of tartrate. Mr. Felix Weber, in a paper on comparative trials of antimony preparations, laid before the Chemical Committee of the Industrial Society of Mühlhausen, pointed out the applicability of the antimony oxalate in every case. Noelting accounts for Hirzel's unfavourable results by the large quantities of oxalate used in the trials—viz., 10grm. and 19grm. per litre, whilst 5grm. is quite sufficient, and is supported herein by the experience of colourists, who find that such strong solutions yield worse results than weaker solutions, and that, specially in steam colours, the colours associated with tannin suffer. The statement of Hirzel's that the colours fixed by the oxalate are less brilliant and fast than those fixed by tartar emetic, is contrary to the experience of Mühlhausen. The separation of insoluble basic antimony oxalate complained of by Hirzel has no disadvantage, since its precipitate fixes the tannin perfectly. Trials laid before the Chemical Committee by Mr. A. Kesters, repeated and confirmed by Mr. Weber, prove that freshly-precipitated antimonious oxide suspended in water, causes a complete fixation of tannin. Noelting does not expect that a few unfavourable results will cause the antimony oxalate to be abandoned, after having become of general and extensive use among the printers of Mühlhausen, and winds up his reply with the advice to practical men—"Try for yourselves."

This advice is to be taken to heart by printers and dyers most particularly. In these arts we have to deal with conditions and forces of which our knowledge is at best hazy, and speculations based upon such foundation can only become of value when tested by experiment.

As regards the fixation of colour tannates by tartar emetic, it is asserted that the antimony of the latter combining with the tannate forms an insoluble compound, an antimony lake similar to baryta being precipitated by sulphuric acid. That this is not so, but that other forces, perhaps attraction of the fibre, come into operation is evident from the following observations:—

A solution of tartar emetic, ten to 1000, is precipitated by a strong tannin solution, one to five, but not by one of one-tenth that strength.

A solution of the oxalate, fifteen to 1000, containing the same amount of antimony (see analysis below) is still precipitated by the weaker tannin solution. But a solution of three to 1000 is precipitated only after some time.

The colours, at least some, combine with the antimonious oxide, forming insoluble compounds in solutions of both the oxalate and the tartrate, but the limit of dilution is in this case even less favourable than with tannin. The presence of gum does not assist the separation of the antimony tannate. It is, however, easy to precipitate every trace of antimony from the weakest solution of the oxalate or tartrate by means of tannin, and *vice versâ*, by adding a small quantity of some neutral salt—for instance, sodium chloride. This precipitate is insoluble in water and in an excess of the antimony solution, but soluble (and this is of great importance to the fixing process) in an excess of tannin. Considering these experiences, I recommend for the fixation of tannin or tannin colours in antimoniacal baths an addition of common salt, either to the colour or to the bath, and expect that from two to five per cent. of the water will be found sufficient.

In conclusion, let me add the result of an examination of the oxalate in crystals, received from Messrs. Koepp & Co.

These crystals are well formed and transparent. In the steam bath they soon become opalescent, then opaque white, with mother-of-pearl lustre, and continue to lose weight for a very long time (14 hours for 1·5grm.). No further loss occurs at 150°, and no oxalic acid is dispelled at that temperature. The composition agrees with that of Souchay and Lossen's salt: $3K_2O$, $6C_2O_3$, Sb_2O_3, $12H_2O$ (*Ann. Pharm.* 105, 250), with the exception of water, which amounts to only 2·57 molecules, as the following tables will show:—

	Found.	Calculated for.		
Antimonious oxide ..	25·83	Sb_2O_3 ..	292 ..	25·51
Potass	23·13	$3K_2O$ ..	282 ..	21·69
Anhydrous oxalic acid	38·11	$6C_2O_3$ ..	132 ..	37·82
Water (loss at 150°)....	11·90			
	99·97			

DISCUSSION.

Mr. Mellor said that his attention had been directed to the oxalates of antimony about six years ago, and since then more than a hundred samples had been tested in his laboratory. The first samples contained 12·5 per cent. of antimony (estimated as metal), later samples contained about 18 per cent., but now they could be bought containing 22 to 22½ per cent. Speaking in round numbers, the percentage of antimony estimated as metal, and not as oxide, of the three salts of antimony mentioned by the author, were antimony oxalate (first quality) 22 to 22½ per cent., tartar emetic 35 to 35½ per cent., and antimony fluoride 39 per cent. Inasmuch as it was the antimony contained in any of these salts which served to fix aniline greens and blues, the buyer had in these percentages a standard of value for the article he was using. Considerable annoyance and loss had occurred to printworks managers from the sale to them of the oxalate as true tartar emetic, and its indiscriminate use in all styles of work. He would therefore strongly urge them not to use tartar emetic substitutes in any prints or combination of patterns which involved the use of ultramarine blue, dove, or brown colours, but with all other colours the substitutes would serve with equal advantage, always provided that the percentage of antimony was equal to that contained in tartar emetic.

Mr. **Walker** had found by experience that it is best not to use the oxalate to fine greens and reds in combination, but for other colours the results had been very satisfactory.

Mr. **Levinstein** felt that the question whether antimony oxalate could in practice replace tartar emetic, weight for weight, in fixing tannate lakes or materials containing tannic acid, notwithstanding that the tartar emetic contained nearly twice as much antimony oxide, was not alone of interest to dyers and printers, but also to English manufacturers of antimony compounds. So far as he was aware, the oxalate was not made in this country, and no doubt its manufacture would be gladly taken up if English makers could be sure of a market. Opinions were, however, divided as to its action. Noelting, and with him O. N. Witt, asserted, as they had heard, that the oxalate could fully replace the tartar emetic, not only for fixing tannate lakes, but also in uni- or single-colour dyeing. On the other hand, Hirzell holds the opposite view, and his experiments had been in some degree confirmed. Noelting tried to explain Hirzell's failure by attributing it to the use of too concentrated solutions. Clearly, then, this divergence of views pointed conclusively to the necessity for further investigation. Did equal quantities of oxalate really replace equal quantities of tartar emetic; and if so, could the former be used in all instances? According to Witt, oxide of antimony in the form of oxalate fixed tannate lakes more easily, or, in other words, the oxalate dissociated its antimony oxide more readily than tartar emetic. It was well known that in one case the residual product of decomposition was acid potassium oxalate, and in the other acid potassium tartrate. What was the action of these residuals on colours? In uni-colour dyeing, where fabrics were first impregnated with tannic acid and immersed in tartar emetic, it might be practicable to keep the latter in permanent use, unless Witt's contention, that antimony tannate dissolved more easily in acid potassium tartrate than in acid potassium oxalate, were proved to be just. Even this, however, was not conclusive, as it would be quite possible to find means to assist in the dissociation of the salt, or counterbalance the injurious action of the residual acid potassium tartrate. These points should be set at rest, and in the meantime he would urge dyers who still adhered to tin preparations, to make themselves better acquainted with the action of antimony compounds on tannic acid or sumac, by which undoubtedly more brilliant lakes were formed than by either stannate of soda or muriate of tin. It might be inferred from the author's paper that Messrs. Koepp first introduced this oxalate. This was, however, not the case, as the salt had wittingly or unwittingly been in use in England for years. To Messrs. Koepp, however, must be granted the credit of being the first to produce and sell it under its proper name.

Dr. **Grossman** quite agreed with Mr. Levinstein as to the importance of proving the comparative values of oxalates and tartrates, and thought that there was no one better qualified for the task than the author. Many years ago Dr. Gerland had published a volumetric method for the quantitative determination of tannin by means of tartar emetic, and had then pointed out the influence exerted by neutral salts upon the precipitation. Having regard to this influence of neutral salts on the precipitation of tannin and antimony, it seemed to him that the divergences of opinion mentioned might arise from the fact that, in the recorded experiments, various disturbing ingredients had been present, and influenced the reaction. Whatever might be the result of the discussion, he trusted that chemists would not lose sight of Dr. Gerland's quick and reliable method of estimating tannin, which hitherto had not met with the recognition which it deserved.

Mr. **Watson Smith** drew attention to the fact that in the August number of this Journal, page 493, an abstract of a paper in the *Bulletin de la Soc. Ind. de Mulhouse* (1885, 318), appeared, entitled, "The Use of Oxalate of Antimony in Printing." This paper, by E. Jacquet, contained directions for the use of antimony oxalate in printing, and these were of value as arising from the practical experience of a Mühlhausen expert.

The **Chairman** thoroughly agreed with Mr. Levinstein in urging the necessity of further experiments to determine the efficiency and economy of the rival salts.

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ON THE VALUATION OF CRUDE NAPHTHAS OF COMMERCE.

BY GEORGE E. DAVIS.

Crude naphtha—first runnings, or the light spirit of coal-tar—is the raw material from which most of the purified benzols of commerce are prepared, and as it varies very much in quality, it is generally sold by the indications yielded by some chemical test.

The method usually employed is to distil a quantity (generally 100cc.) in a tubulated glass retort, and to catch the quantity distilling over, up to a temperature of 120° C., the bulb of the thermometer being immersed the whole time in the boiling liquid. What is considered an average sample of crude naphtha should yield, on distillation, 30 per cent. at 120° C., and this is generally the basis on which this article is sold, so that contracts are usually made at so much per gallon for 30 per cent., with a rising or falling allowance per degree (or every one per cent.) within reasonable and pre-arranged limits.

Crude naphtha varies in quality according to the source of the tar from which it is distilled, and writing from the examination of a large number of samples extending over upwards of ten years, and from the results of working on the large scale, I can confidently say that the foregoing method of testing is both illusory and unsatisfactory. Sellers may prefer the test on account of its simplicity, but buyers of crude naphtha and rectifiers of benzol usually have some secret method of their own, which they employ when offering for a parcel by sample, and so are often able to avoid making a doubtful purchase. In the case of contracts it is different—if say twelve monthly deliveries have to be made, each parcel may vary widely in its composition to the detriment of the purchaser, and yet the seller be well within the four corners of his contract.

Such a state of things needs reforming, and now that crude naphtha is practically unsaleable, this seems the time for inaugurating a change. It must not be forgotten, however, that the sellers of crude naphtha and the refiners of benzol are not always brought into contact with each other. Most of this business is in the hands of agents, who write round to sellers for samples of what they have to dispose of. These samples are tested (generally by a lad in the office) for what they will yield at 120° C., and offers are then requested from the refiners. Here then is an example of what all traders should do—buy in the cheapest market and sell in the dearest—if they can do so. The refiner would be able, in most cases, to draw his own inferences, if a sample was sent with the inquiry, but this is not always done, the test alone being sent.

In the case of no sample being sent, the purchase of a parcel of crude naphtha is a very risky thing, and can only be dealt with commercially, by allowing sufficient margin to cover the risk—this is the seller's loss. On the other hand, it is not the only quantity to be taken into consideration, as even with samples before them distillers vary exceedingly in the prices they offer for one and the same parcel. No doubt this is in a great measure due to various methods of valuation, and if the lowest value offered yield but an honest profit, the highest must be a failure. I say a failure guardedly, not inferring an absolute loss, as a rectifier may have to make future deliveries at good prices, which may warrant him offering more than his neighbour; but if he pays for his crude material more than the market price of the day at the moment of purchase, he is not a shrewd financier.

Crude naphtha is valuable only for the benzene, toluene, and solvent naphtha it contains, so that an adequate method of valuation should take them all into consideration. The distillation test to 120° C. will not differentiate these products, nor will it give any idea of what can be positively realised on the large scale. A considerable portion of the bulk, too, is often eliminated during the washing with acid and soda, and of this varying quantity the test takes no account. How can it then stand its ground?

As an average of many samples from different parts of England, the following distilling points are given:—

Sp. Gr.	100°	110°	120°	140°	170°	200°
·905	2	11	33	57	80	92

The distillation test at 120° C. could not be applied in the case of a very rich naphtha, such as that obtained from the gas in the carbonisation of coal, for in this case there is but a small proportion of "solvent," of which the rectifier expects to get his due share.

The average test of a large number of samples of crude benzol, obtained by my process at the Rockingham Gas Works, gave the following test, showing how little "solvent" there is in it:—

Sp. Gr.	100°	110°	120°	140°	170°	200°
·890	61	83	90	96	98	99

There is another class of benzols or naphthas often requiring valuation—viz., "twice-run naphtha," which term may mean almost anything, and yield products even more various in their character than the products direct from tar. Some makers of it collect the lighter portions in one tank, and the "tailings" in another, the first, being richer, is sold by sample, the second generally goes to those who have been foolish enough to contract exclusively for the 120° C. test. The following are some distilling points of this class of naphthas:—

Sp. Gr.	100°	110°	120°	140°	170°	200°
·878	0	17	41	72	91	99
·888	3	23	46	71	88	95
·901	2	20	40	62	80	90
·901	0	9	29	57	81	95

It may be inferred, with safety, that the simple specification of so much per cent. distilling at 120° C. is no guide whatever to the amount of valuable products the rectifier can get from a parcel.

Some years since I was asked to verify the certificates of a number of deliveries of crude naphtha by means of a specified test, which, being much better than that just described, I will give in detail:—100cc. of the naphtha to be distilled in a 200cc. flask, with a thermometer arranged so that the bulb shall be just off the bottom of the flask. The distillate to be caught in a 100cc. graduated measure, and the yield at the following points taken:—100°, 110°, 120°, 140°, 170°, 200°. The quantity which shall have passed over at 140° C. is to be again distilled, and the yield at 100° C. and 120° C. noted. All which passes over up to 100° C., in the second distillation, is called *Benzol*; that which passes between 100° C. and 120° C., in the second distillation, is called *Toluol*. *Best Naphtha* (N_1) is that which passes between 120° C. in second distillation and 170° C. in the first. *Second Naphtha* (N_2), that caught between 170 C.—200° C. in the first distillation; and *Creosote* (C), the difference between 100cc. and the yield at 200° C. in the first distillation.

The following are some tests, by this method, of samples which passed through my hands at that date:—

Sp. Gr.	First Distillation.						Second.		Products.				
	100°	110°	120°	140°	170°	200°	100°	120°	B	T	N_1	N_2	C
·886	2	22	42	67	88	94	23	53	23	30	35	6	6
·893	11	30	50	72	88	95	27	56	27	29	32	7	5
·903	2	11	23	56	78	92	23	42	23	19	36	11	8
·915	0	2	11	29	73	94	7	26	7	19	47	21	6
·917	2	16	31	57	75	87	22	43	22	21	32	12	13
·910	2	10	21	42	59	69	23	36	23	13	6	17	10

Now although this method appeared to suit certain buyers it seemed to me necessary to examine a large quantity of these products, and for this purpose five bottles were used to collect the different fractions from many samples. The distillation of them showed as follows :—

B	85°	90°	95°	100°	110°	120°
	2	48	71	80	90	95
T	100°	110°	120°	130°	135°	
	47	75	86	91	96	
N_1	130°	140°	150°	160°	170°	180°
	13	43	63	79	91	93
N_2	160°	170°	180°	190°	200°	
	2	31	55	71	87	

The creosote in the fifth bottle crystallised into a solid mass. The above shows clearly that the method is not exact enough for times of great depression and minimum profits, and, moreover does not indicate truly the maximum price which may be offered to the seller. The "benzol" is only of 80 per cent. quality, while the "toluol" is nearly a 50/90 benzol. The naphthas (N_1 and N_2) are but mixtures of solvent and light oil, which have to-day very different valuation figures.

As a comparison to the foregoing, I have lately applied the method to the valuation of the crude benzol produced from coal gas, and have arrived at the following figures :—

B	85°	90°	95°	100°	105°	110°	120°		
	42	78	88	92	95	96	dry.		
T	100°	110°	120°	130°	135°	145°			
	45	71	84	90	93	95			
N_1	115°	120°	130°	140°	150°	160°	170°	180°	190°
	4	15	36	52	63	72	78	83	88
N_2	140°	150°	160°	170°	180°	190°	200°		
	4	17	30	43	56	68	78		

The creosote was a completely solid and hard cake.

These tests show that although the methods usually employed may give the distiller some faint guide of the quantities to be expected from the still, they can scarcely be called scientific, and for one good reason, that they do not take into account the products eliminated during the washings with acid and soda, the loss in these operations being often very great. To take a case in point—1500 gallons of crude naphtha (for sake of identification we will call it HB) tested :—

B	T	N_1	N_2	C
25	23	32	10	10

This quantity, after washing with ROV and soda, measured 1200 gallons, and tested :—

B	T	N_1	N_2	C
26	14	24	9	27

When this quantity was worked in a still of the best construction, the yield was :—

90% benzol .... 318 gallons = 23%
50% benzol .... 362 " = 24%
Solvent naphtha .... 150 " = 10%

The test would therefore lead us to suppose that much more solvent naphtha could be obtained than was actually the case.

In another instance, 1500 gallons of crude benzol from gas of our own make, tested :—

B	T	N_1	N_2	C
70	6	9	4	11

After washing with R.O.V and soda it measured 1440 gallons, and tested :—

B	T	N_1	N_2	C
69	5	9	4	11

When worked in the still it yielded :—

90% benzol .... 1166 gallons = 77·7%
Solvent naphtha.... 33 " = 2·2%

Here the test indicates much nearer the yield on the large scale than with crude naphtha; but there is very little, if any, of the crude benzene from carbonising to be found in the market.

The method I have found to be of great service in the valuation of crude naphthas is as follows :—200cc. of the crude naphtha are agitated with 20cc. of concentrated oil of vitrol in a globular, stoppered separating funnel of 300cc. capacity. After agitating well for five minutes, allow to settle, and run off the acid carefully so that nothing is lost. (It is better sometimes to give two washings with R.O.V. of 10cc. each, instead of one washing with 20cc.) Now wash twice with 30cc. of water each time, running off the wash waters very carefully, then add 30cc. caustic soda solution (sp. gr. 1060) and again wash with water. Pour the washed naphtha into a measuring jar and determine the percentage loss. All is now ready for the distillation. Measure off 100cc. of the washed naphtha, less the percentage loss (thus if the loss has been 10 per cent. 90cc. will have to be taken, and so on) and place in a 200cc. flask fitted with a delicate thermometer and Glinsky's or Le Bel Henniger's fractioning tubes, connecting it with a Liebig condenser of the ordinary pattern. Heat the flask with a rose-burner, and so regulate the flame that a drop distils over about once in every two seconds. Carry the distillation on until the thermometer has risen to 120° C., catching the distillate in a graduated jar. At this point change the receiver, and catch all that comes over up to 170° C.

The distillate to 120° C. consists substantially of benzene and toluene; that portion distilling between 120° C. and 170° C. is solvent naphtha, the remainder creosote. The "solvent" produced in this small apparatus will show first drop at 125° C. and 90% at 160° C., which is usually the quality worked for on the large scale.

The composition of the distillate to 120° C. will vary somewhat with every sample of crude naphtha operated upon. In my own case, the quantities yielded by each analysis were collected from many samples, and when mixed and tested in the usual way employed for 90% and 50% benzols, showed 64% at 100° C. and 93% at 120° C., and I have found this method so handy in practice, that it is easy, without any supervision, to run the specified quantities of 90's and 50's from the still direct, without any fear of mistakes.

When operating upon "crude benzols" from coal-gas, the quality is much higher, and generally runs

	Sp. Gr.	OLD METHOD.	IMPROVED METHOD.					NEW METHOD.				
		% at 120° C.	B	T	N_1	N_2	C	Loss.	% at 120° C.	Test at 100° C.	% Solvent.	Creosote.
Crude Naphthas	·886	42	23	30	35	6	6	13·5	50	60%	23	27
	·902	31	19	22	35	15	9	18·5	46	59	19	36
	·908	29	17	23	39	13	8	10·5	45	61	23	32
	·921	32	30	13	27	15	15	16·5	47	58	13	40
Crude Benzols.	·885	89	80	5	11	2	2	0·7	82	91	2	16
	·895	77	70	6	9	4	11	7·5	77	92	2	21
	·900	71	61	9	9	6	13	8·5	71	93	4	25
	·909	61	55	12	10	3	10	7·0	67	91	6	27

94% at 100° and 98% at 115° C. The results of the older methods, as compared with the new, are given in table on previous page.

In order to rapidly estimate the quantity of 90%, 50%, and toluol which may be run from the still, I have constructed the following table:—

MIXTURES OF 90% BENZOL WITH 50/90 BENZOL.

90%	50/90.	First drop.	% at 100° C.	% at 120° C.
0	100	92°	50	90
5	95	92	51	92
10	90	91	51·5	92
15	85	91	53	92
20	80	90	55	92
25	75	90	60	93
30	70	90	65	93
35	65	90	67	94
40	60	88	69	94
45	55	88	70	94
50	50	87	71	94
55	45	87	73	94
60	40	86	76	95
65	35	86	78	95
70	30	86	79	96
75	25	85	81	96
80	20	85	84	97
85	15	84	86	97
90	10	84	88	dry.
95	5	84	89	dry.
100	0	84	90	dry.

MIXTURES OF 50/90 BENZOL AND COMMERCIAL TOLUOL.

50/90	Toluol.	First drop	% at 100° C.	% at 105° C.	% at 110° C.	% at 120° C.
100	0	92°	50	68	80	91
95	5	93	45	64	76	91
90	10	94	33	60	73	90
85	15	94	30	58	73	90
80	20	95	28	57	72	90
75	25	95	26	55	71	90
70	30	96	22	48	67	90
65	35	96	19	47	65	90
60	40	96	15	40	65	90
55	45	97	12	41	65	90
50	50	98	8	42	61	90
45	55	98	4	31	57	90
40	60	99	0	26	56	90
35	65	100	0	25	55	90
30	70	100	0	23	53	90
25	75	100	0	21	53	90
20	80	100	0	16	48	90
15	85	101	0	14	46	90
10	90	102	0	13	45	90
5	95	102	0	10	44	90
0	100	103	0	4	39	90

MIXTURES OF 90% BENZOL WITH COMMERCIAL TOLUOL.

90%.	Toluol.	First drop	100° C.	105° C.	110° C.	120° C.
0	100	103°	0	7	50	91
5	95	102	0	21	59	91
10	90	101	0	30	66	94
15	85	100	0	38	68	95
20	80	97	9	48	73	95
25	75	95	16	53	76	95
30	70	95	23	59	78	96
35	65	94	33	60	80	96
40	60	93	43	66	82	96
45	55	92	46	71	85	97
50	50	91	52	72	86	97
55	45	91	58	76	86	97
60	40	91	60	78	88	97
65	35	90	65	81	89	97
70	30	89	71	83	91	97
75	25	88	75	85	91	98
80	20	87	77	87	92	98
85	15	86	83	89	93	98
90	10	85	85	91	94	dry.
95	5	84	88	93	95	dry.
100	0	84	90	94	96	dry.

Thinking it may be interesting to have the distilling points of 90% and 50% benzol, made with pure benzene and pure toluene, I have made the following mixtures with great care, and from materials prepared as pure as it is possible to make them:—

Pure benzene, collected from a large still, crystallised in the Linde ice machine, partly melted and filtered, and again rectified, collecting only the middle portion. It possessed a gravity of ·884, at 15°·5 C., distilled at 80°·5 C. to 81° C., the thermometer falling again to 80°·5 C. near the end of the distillation. When distilled with open steam, the whole came over at a constant temperature of 69° C. (the thermometer in the vapour), the ratio of benzene to water being 16 to 1.

Pure toluene. Prepared in the toluene rectifying apparatus, and re-rectified several times. It had a sp. gr. of ·871 at 15°·5 C., and distilled over at 110°·8 to 111° C. When distilled by open steam the whole came over at 84°·5 C. (thermometer in the vapour), the proportion of toluene to water being 5 to 1.

Pure xylene. Prepared from solvent naphtha on the large scale, and rectified by means of Glinsky's tubes in the laboratory. It came over between 137° C. and 140° C.

The following mixtures were found to produce results as follows:—

	90% benzol.	50/90 benzol.
Pure benzene	75	50
„ toluene	24	40
„ xylene	1	10

and their distilling points for every degree, when distilled in the ordinary manner used for commercial benzols, were as set forth in the table on next page.

Now as to valuation. It is very evident that the old method based on so much per cent., distilling at 120° C., is extremely variable. When benzene, toluene, and the lighter portions of solvent naphtha are equal in price, then, and then only can the method approximate to its real value, but it *never* can be accurate, as crude naphthas without exception, contain very large and variable percentages of light and heavy hydrocarbons which are removed by treatment with strong vitriol and soda. It is nothing more nor less than a lottery to buy by such a test.

In the second, or improved method, the valuation is simplified, but the B, T, N_1 N_2 and C as proved by their distillation points are not of the quality usually known in commerce, and, moreover, the same fault exists as with the old test—viz., that the test is made upon the unwashed and indefinite article. Thus there is a certain amount of risk, and this must occasion a lower price being offered to the seller to cover that risk.

° C.	90%	50%	C.	90%	50%
87	First drop.				
88	10		105	94	68
89	18		106	—	70
90	29		107	—	71
91	56	First Drop.	108	95	73
92	70	—	109	—	75
93	71	2	110	95½	78
94	76	11	111	—	79
95	80	22	112	—	82
96	82	29	113	96	83
97	85	31	114	—	84
98	87	43	115	—	86
99	88	46	116	—	87
100	90	50	117	97	88
101	91	55	118	—	89
102	91½	57	119	—	90
103	92½	61	120	98	91
104	93	64	..	..	..

The valuation figures will, of course, depend upon the market prices of the finished articles, though few distillers would be able, without having practically worked many parcels, to say definitely what margin should be allowed between the purchasing and selling prices of the various constituents.

In 1875, when the prices of the finished articles were as follows:—

Benzol, 90% 6/-
" 50% 5/-
Toluol 2/6
Solvent (N_1) 2/-
" (N_2) 1/6
f.o.b. Goole, casks free, less 2½% cash in 14 days.

The values for the analytical products were as follow:—

B	T	N	N_2	C.
4/-	2/6	1/6	1/-	1d.

So that a crude naphtha, such as that alluded to as H B, and worked in the still, would have been worth:—

$$\begin{array}{lr} & \text{d.} \\ \text{B} \quad 25\times48\text{d.} = & 1200 \\ \text{T} \quad 23\times30\text{d.} = & 690 \\ N_1 \quad 32\times18\text{d.} = & 576 \\ N_2 \quad 10\times12\text{d.} = & 120 \\ \text{C} \quad 10\times1\text{d.} = & 10 \\ \hline & 2596 \end{array}$$

$$\frac{2596}{100} = 2/2 \text{ per gallon.}$$

To-day the selling prices are—

90%	50%	Toluol	Solvent	Creosote
1/7	1/1	1/2	10d.	no value

and the values of the analytical products:—

B	T	N_1	N_2	C
11d.	8d.	6d.	1d.	nil

making the value of such a crude naphtha to-day, by this method, but 6½d. per gallon.

The figures obtained by my method are absolute and comparable, from whatever part of the country the crude material is derived, the percentage loss, and the heating which takes place on treating with acid gives one a very good idea of the quality of the stuff one has to work, while the distillates represent very faithfully the quantities of benzol and solvent naphtha to be obtained on the large scale. Take, for instance, the H B sample again—by the new test it gave:—

Loss by washing 18¼%
Distillate to 120° C. 45%
Test of same ...... $\frac{100°}{71}$ $\frac{120°}{94}$
Distillate to 170° C. 10%
Test of same ...... $\frac{126°}{0}$ $\frac{160°}{92}$

The quantities actually obtained from the still were:

90% benzol 23%
50% " 21%
Solvent 10½%
Test of solvent .... $\frac{130°}{1}$ $\frac{160°}{94}$
Loss by washing 20%

Knowing this, it would be easy to make an offer for any parcel, with the certainty of knowing that the laboratory results will be produced on the large scale, and the test may be made one in which both buyer and seller could agree upon a sliding scale, the price f.o.b. Goole, or d/d in London, being made the basis. Of course, the percentage of the selling prices each manufacturer could afford to give would vary according to circumstances. One with a good pattern of still and a careful rectifier working it, would be able to offer more than one using an old boiler for a still, and injecting steam *ad lib.* into the contents. One may have cheap vitriol for washing, cheap labour, or cheap carriage, which another would not possess, and all these things would have to be taken into consideration.

A rectifier of crude naphthas should be able to say, "I know by the test what I am able to get out of the parcel, and will offer so much per cent. of the selling price, calculated naked at my works, for the 90's, 50's, and solvent contained therein;" the seller delivering the naphtha to the purchaser's works free, the barrels to be returned free when empty. Some manufacturers are able to do this; it is for sellers to know it can be done in a satisfactory manner. Now, taking this selfsame crude naphtha again, let us see what money values were obtained from it.

90% benzol .... 23gals. at 1/7
50% " .... 21 " at 1/4
Solvent .... 10 " at 9d.
f.o.b. Goole casks free, less 2½% cash in 14 days.

But we must deduct 2d. per gallon for barrels, 2d. for carriage, and 1d. for sundry charges, as leakages, super tares, etc., leaving naked at the works:—

$$\begin{array}{lr} & \text{d.} \\ \text{90\% benzol........ 23gals. at } 1/2 = & 322 \\ \text{50\% " .... 21 " at 11d.} = & 261 \\ \text{Solvent .... 10 " at 4d.} = & 40 \\ \hline & 623 \end{array}$$

$$\frac{623}{100} = 6\tfrac{1}{4} \text{ per gallon.}$$

Out of which the rectifier will have to find vitriol, soda or lime, wages, steam, repairs, depreciation and other sundry charges, and his profit if there be one.

The present state of things is most unsatisfactory, both for buyer and seller, and if this paper tends to bring about a better state of things it will not have been written in vain.

Newcastle Section.

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J. F. Stark.
T. W. Stuart.
Local Secretary and Treasurer: J. T. Dunn, 115, Scotswood Road, Newcastle.

Notices of papers and communications for the meetings to be sent to the Local Secretary.

Meeting held November 4, 1885, in the College of Science.

MR. B. S. PROCTOR IN THE CHAIR.

NOTE ON THE COMPOSITION OF GAS FROM MESSRS. ALLHUSEN'S BORINGS AT MIDDLESBROUGH.

BY PROFESSOR P. PHILLIPS BEDSON, D.SC.

The two samples of gas, the composition of which forms the subject of this short note, were collected from two distinct outbursts of gas which occurred last July during the piercing of the salt at the second of Messrs. Allhusen's borings at Middlesbrough. A similar outburst of gas took place during the piercing of the salt at the first of the borings, the gas issuing from the bore hole in large volumes, shooting into the air, water, mud and debris, to a height of several feet.

For the opportunity of examining these gases and for their collection I am indebted to the kindness of Mr. A. Allhusen.

The gas was collected by placing in the bore hole, at the time of the outburst, an iron tube some 6ft. in length and about 2in. in diameter. This tube was connected by indiarubber tubing with a large glass vessel, such as is used in the construction of aspirators, filled with water. The iron tube was connected with the upper opening of the aspirator, and, by allowing the water to flow out, the gas was drawn in.

The salt was reached at a depth of about 1000ft., and after it had been pierced to a depth of some 82ft., the first outburst of gas occurred. The gas collected proved to have the following volumetric composition:—

Oxygen	1·53
Marsh gas	1·90
Nitrogen	96·57
	100·00

The second outburst occurred at a depth of 106ft., the collected gas having the following volumetric composition:—

Carbon dioxide	0·3
Oxygen	2·9
Nitrogen	96·8
	100·0

From Mr. Hugh Bell's paper, "On the Salt Deposits of Middlesbrough," read before the Cleveland Institution of Engineers in April, 1883, it will be seen that the salt of these deposits contain vessicles, locked up in which is gas, the liberation of which, on the solution of the salt, is frequently productive of slight explosions. This gas is said to consist of nitrogen and oxygen, in the proportion of 94vols. of the former and 6vols. of the latter. The Middlesbrough salt, therefore, resembles certain varieties formed in the Wieliczka deposits, which exhibit similar decrepitations; the gas contained in them being, however, marsh gas only. Further, Mr. Bell mentions in his paper that, during the stoppage of the pumping at Clarence, large volumes of gas bubbled up at the fresh-water inlet of the pumping machinery. This gas proved to contain 97·9vols. of nitrogen and 2·1vols. of marsh gas. Now supposing the oxygen in the gas obtained at the first outburst, to represent air admitted in collecting the gas, the residual gas would contain the following proportions of marsh gas and nitrogen:—

Marsh gas	2·05
Nitrogen	97·95
	100·00

Showing it to be identical in composition with that described by Mr. Bell. If the results of the analysis of the gas obtained at the depth of 106ft., be interpreted in a similar manner, the residual gas would have the following volumetric composition:—

Carbon dioxide	0·4
Nitrogen	99·6
	100·0

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Local Secretary:
G. G. Henderson, Chemical Laboratory, University of Glasgow.

Notices of papers and communications for the meetings to be sent to the Local Secretary.

The opening Meeting of the Third Session of this Section was held in the Rooms, 207, Bath Street, on November 3, 1885.

CHAIRMAN'S OPENING ADDRESS.

Mr. J. Neilson Cuthbertson said: It has appeared to me, in considering the many subjects that present themselves as suitable for an address, that, in my capacity as a member of the School Board, I might most appropriately refer to the present position of scientific and technical education for the masses of the people. You are probably all aware that four years ago a Royal Commission was issued to inquire into the instruction of the industrial classes in technical and other subjects, with power to take evidence and visit foreign countries. The report of the Royal Commission has been in the hands of the public for more than a year; I have no doubt you have a copy of it in your library, and it must be known to many of you. But, believing the subject to be of such importance that too much attention cannot be given to it, I venture to direct some thoughts to the conclusions of that report. The Commissioners say that they have visited almost every country in Europe, and also in the United States of America; and although they had high expectations of what they should find there, their expectations were exceeded, and there is no doubt, they say, that many and great advances have taken place in Continental nations in the matter of technical and scientific education. I need only refer you to the most recent development of that education in the splendid university which has been erected at Strasburg. I suppose this new university is expected to take a leading place amongst the universities of Europe. Probably no other university can equal it, with its ninety-two professors, its large number of students, and immense appliances in the way of apparatus and library. We can only hope that that university will have a future in keeping with the splendid preparations that have been made for its success. I might refer, as the Commissioners do, to the great success that has attended Continental science in different ways; in organic chemistry, in the introduction of the ammonia process in the alkali trade, in the ventilation of deep mines, in the application of water power, in the invention of the dynamo machine, in the construction of roofs and bridges, in the printed cottons of Mulhouse, in the woollen yarns of Verviers and in the ribbon trade of Basle. All this is largely due to the greater painstaking, to the greater pliancy and thrift of those concerned, and also to the general acquaintance with modern languages usually possessed by Continental manufacturers.

The reporters go on to say that notwithstanding the great advance that has taken place in these foreign nations, on the whole our people still maintain their position at the head of the industrial world. You must all have been struck with the recent addresses of Sir Lyon Playfair at Aberdeen and Dundee, and how completely he places foreign nations ahead of our own in this matter of scientific education. I am inclined to believe, however—and this report bears me out—that we are not so far behind after all. I think Sir Lyon's words may be taken to mean that we are all right up to a certain point, but require to take care of our hand in time to come. In proof of this position the Commissioners cite the important inventions that have been brought out in this country during the present and the past generations; in fact, they go the length of saying that all the principal machinery that is now at work on the Continent has been derived from England, or, at all events, is the direct offspring of suggestions made in Great Britain. Even in those branches where other nations have hitherto excelled, the reporters are of opinion that we are improving. They maintain—and I hold correctly—that the beginnings of the modern industrial systems are due in the main to Great Britain. Other nations were greatly hindered during the latter part of the eighteenth and the beginning of the nineteenth century by the wars which devastated Continental Europe, and so jealous was the nation at that period and at the close of the great war, that legislative restrictions were imposed which hindered our people from going abroad, and even put a hindrance in the way of exporting machinery to Continental countries.

This was no doubt the occasion of the great efforts put forth by Continental nations to cope with this country, and they began by instituting Polytechnic Schools. These schools are all over the Continent. There is *L'Ecole Centrale* at Paris, and there are the Polytechnic Schools of Germany and Switzerland; and the first step they took was to get men from England and Scotland to start them. After that sprang up the Technical High Schools, for a lower grade of instruction, inaugurated in most of the Continental States. These two grades of schools have been kept mostly at the expense of the different States—I mean the Governments. The result is that everyone who has had to do with Continental manufacturers must have noticed the greater intelligence of their chief men. A Continental manufacturer is generally a man of education himself, and thoroughly up to all the new inventions of the day. He is generally acquainted with several languages, and has thereby the opportunity of studying the periodicals of this country and the literature of the day all over Europe, and of picking out the inventions and appliances most suitable for his own trade. More recently there have been instituted in many of the States Technical or "Industrie-Schulen," intended for boys and young men who wish to become foremen, or otherwise engaged in industrial occupations. The instruction in these schools is similar in character to that of the higher instruction, but inferior in degree. In France they have taken a very large development and are generally treated as higher elementary schools, to which the cleverer boys are passed on after they have completed the curriculum in the ordinary schools. This idea of picking out the cleverer boys is common to the schools in America as well as to those of Continental Europe. They don't bring on the dunces to the higher schools; they examine each scholar after the curriculum has been gone through, and if he can't pass a creditable examination, he is not passed on to the higher school unless at his own expense. Notwithstanding all these efforts, the reporters say that foremen and workmen are not, as a rule, up to the present time, technically instructed, and that they have not—even with all the efforts that have been made—greater advantages than our workmen or foremen have in this country. This class of effort has been chiefly maintained by local effort, and largely by the manufacturers themselves, who seem to be quite aware of the necessity nowadays of scientific instruction, even amongst their men, at all events amongst their foremen, and they spare no pains to have them educated. Nothing, however, that they have yet been able to achieve is equal to the instruction that we have in this country through the schemes of the Science and Art Department and the City and Guilds of London Institute.

In two respects scientific education abroad seems to excel what we have at home. First, as regards *drawing*; and second, as regards *general elementary education*. In France and Belgium an effort is made to have every child instructed in drawing; and in regard to the United States, the Commissioners consider the great feature of technical instruction there is the superior general education, which the mass of the community receive in the elementary schools.

I need not say that this latter deficit will not much longer exist, for I maintain that the mass of our people, by another generation at all events, will be as well, if not better educated than their American cousins.

Now, to bring home these remarks to our own country, the reporters wish to point out the need we have of technical instruction, especially for foremen and artisans. The time was when it was supposed that instruction of that kind had better be reserved for the head of the house, with one or two assistants in the laboratory, but I hope that day is at an end. We have now our mechanics' institutions blossoming into colleges of science and arts. We have evening classes everywhere for science of a kind. I don't pretend for a moment that the science thus to be had is of the very highest level; at all events it goes so far as to accustom young people to elementary ideas regarding science, and to bring out (which I think is a great point) those who have the aptitude and abilities for following out the higher studies. In proof of this, I may cite the opinion of a gentleman well known to you all, who was called upon to examine a school in elementary science. I said to him, "Now, do you think that what you have seen there is likely to interfere with your own professional work?" "Not at all," he said; "I believe that all that anyone can do in that direction will only be a feeder to the higher institutions." I think if this view be taken, you will find in future a much larger number of boys who would be desirous to have higher scientific instruction. They point also to the examples of special schools. I am not acquainted with the lace trade in Nottingham, but from the report it seems that Nottingham has been largely benefited by an art school there, and the lace trade which had been languishing has got a new impetus; and it is known to us that the Lambeth School of Art has been the means of raising the Doulton pottery to a higher position than it might otherwise have attained. I need scarcely mention to you, gentlemen, the City and Guilds of London Institute. There we have an effort put forth by the Guilds of London, who are possessed of immense wealth, and who of late years have been looking about to make a good use of it, lest it should be taken from them in another way; and we must certainly approve of what they have been doing. All over the country they hold examinations which have been the means of bringing forward a continually increasing number of candidates. Their rewards are of a very substantial kind, including medals and money rewards; and I have no doubt many working men have been brought out who otherwise would have remained unnoticed. The universities, too—to ascend to a higher platform —now give a much larger modicum of natural science than formerly, and I am quite sure professors of science in universities will cordially welcome any movement in that direction. Indeed, I may say that throughout the whole community there is a growing desire, and especially amongst educational bodies to promote scientific and technical education; and a desire, above all things, to know how it can be best accomplished.

The reporters are of opinion—and I dare say we all agree with them—that the foreign systems are not exactly applicable to this country. There is a kind of autocratic fragrance about them, that could not rightly be transplanted into British soil, and we must look at home for something of a more natural and easy growth. With the exception of France, where the recent educational revival has assumed enormous proportions, the British Government is now spending a larger sum of money than any Government in Europe upon its education, and all that is needed is to see that it be properly and wisely dispensed.

In a general way it may be taken for granted that men of leisure and means have no difficulty in this country in getting scientific education. I may perhaps differ with Sir Lyon Playfair as to this point, for, as far as I remember, in Glasgow there never has been a time that there was not good scientific education. In fact, I have been often astonished to hear people from a distance speak of the difficulty of getting instruction in chemistry. In all my recollection there have always been schools of chemistry in Glasgow, still increasing in usefulness and popularity, so that people of very moderate means have hitherto had no difficulty in getting scientific education. The best preparation, the Commissioners remark, for studies of that kind, is a good system of secondary schools, and they point above all others to the Manchester Grammar School, as a school which has of late years been brought up to a high point of efficiency, and which they recommend as an example to others. I hope that in Glasgow we shall be able, with the assistance of the Endowments Commission and of other local authorities, to have abundance of scientific instruction of that higher grade. But that is not quite the point at which the Technical Commission has been aiming. It is rather how are the great mass of the working population to be instructed in elementary science. There can be no doubt that that ought to be begun in the primary schools. The Commissioners consider that if general ideas of science be given to children of ten, eleven, or twelve years of age (as I stated at the earlier part of my remarks), this will prove to be seed sown in the minds of boys, which will bear fruit in the form of a hunger for greater information; and they think this should be done by special teachers, or by visiting masters, as has been done in Liverpool and Birmingham—going round the schools on certain days of the week giving lessons in elementary science—and then the scholars so initiated should be led up to higher schools or classes, either day or evening. This sort of work involves a set of apparatus, which, of course, must be supplied from some public source. They seem to think that the School Boards would not be able to overtake that, but I have yet to learn that the Scotch Act of 1872 does not include this to such moderate extent as may be required. They also recommend the institution of industrial museums and loan collections, and that all these should be largely promoted by employers and manufacturers. There is no doubt, as I have already said, that what is being done in that connection on the Continent is promoted by employers of labour more than by the Government; and I feel quite sure that great employers of labour in this neighbourhood need only to have their attention called to this matter to be willing to encourage such scientific education among the people whom they employ. It is also recommended that manual work should be introduced into the public schools. Regarding this there may be differences of opinion. It has been done very successfully in Allan Glen's School, in the City of Glasgow, and in the Buchanan Institution, and in some towns in England. The reporters give a list of all the different institutions and colleges they have visited, and while not willing to differentiate between one and another, they are anxious to believe that all are doing good work, and only require greater encouragement and greater support in the way of funds, to do a much better work. They make again and again reference to the technical examinations of the City and Guilds of London Institute. I know scientific men are apt to pooh-pooh this sort of thing, but what I say is that in the absence of anything better we are very glad to have it, and if it be but the beginning of a better state of things we shall have good cause to thank them and to hope that they may continue to prosecute their labours.

At the close of their report the Commissioners make one or two suggestions. In the first place they suggest that in the public schools drawing should be incorporated as a class subject. Drawing does not so much concern us as a society, but still it seems to be applicable to most branches of scientific study, and one good effect the publication of this report has had is, that now the Education Department have issued instructions that drawing shall be taught to every child—boy and girl—and no doubt this will result in much good. They express their own opinion that boys especially should be taught to use tools in wood and iron, and that this should be made a "specific subject" in the education code. They also suggest that the provision which is now operative in Scotland, to prevent a boy or girl under 14 years of age from being employed full time, unless they have passed the fifth standard, should be extended to England. As to classes in connection with the science and art department they consider that the local authorities, school boards, or others, should be empowered to establish such classes. Now in Glasgow we consider we have power to establish such classes, and I hope that before the expiry of the present School Board we shall have nine well filled art schools, with drawing room perhaps unsurpassed by any in the kingdom, and that we shall also have six or eight schools where elementary science is taught with proper apparatus. They suggest that the inspection of such classes ought to be rendered more effective, and that contributions should be made by the authorities at head quarters to local museums, or in some other public way. They are of opinion, as already explained, that a great deal is to be expected from employers of labour and trade organisations. They hope to see the day when it shall be necessary for a boy, before entering on an occupation where science is required, to undergo some preparation in elementary science; and when the managers and promoters of such classes shall place the teachers in a position to carry their boys into higher stages, either by having scholarships introduced, or by some other means.

Now, gentlemen, if you want to get a good crop of scientific men, at all events men intelligent as to the elementary science, you must begin at the lowest stage; not give a boy or girl too much to understand or study, but give them broad general ideas, which shall enable them afterwards to appreciate the higher instruction which many distinguished men in this city and elsewhere are prepared to give.

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FURTHER NOTES ON A NEW SYSTEM OF COOLING OILS FOR THE EXTRACTION OF PARAFFIN.

BY GEORGE BEILBY.

At one of the meetings of this section last session I described a new system of cooling oils for the extraction of paraffin (*Journal*, iv. 321). At that date apparatus on the full manufacturing scale had just been completed at Oakbank Works. It has occurred to me that some of the members may be interested to hear the results of the summer's work with the new plant.

June, July, and August have always been trying months in the freezing houses of paraffin works. Freezing plant, which, for the colder months of the year, is of ample power, may, in the summer months prove quite insufficient. This loss of efficiency necessitates one of two courses, either that a large percentage of paraffin must be allowed to escape in the oils, or that the imperfectly frozen oils must be kept in stock till the colder months to be then refrozen; under either alternative heavy loss is entailed.

The new system was designed to meet and overcome two manufacturing difficulties. The first of which has been referred to, namely, the loss of cooling efficiency during the summer months. The second difficulty is the imperfect separation of paraffin from oils, which results from too rapid freezing, whereby the paraffin is separated in an imperfectly crystalline state, and this amorphous or semi-crystalline paraffin is not only difficult to filter from the bulk of the solvent oil, but the pressed cakes of paraffin scale are apt to retain an excessive percentage of oil. How far the new system has overcome these difficulties will be shown by an examination of the results of the past six months' working.

The Temperature of the Cooling House.

The apparatus and the structure of the house were originally calculated and designed for the maintenance of the air inside, summer and winter, at a temperature of 32° F.

The cooling house is 56 feet long, 48 feet broad, and 25 feet high, its cubic capacity is therefore 67,200 cubic feet, and, after deducting the volume of the contained apparatus, the actual air space within the house is 58,900 cubic feet.

The internal wall surface is 5200 square feet, the roof and floor surface 5376 square feet, or a total internal surface of 10,576 square feet. The cooling of the air of the house is effected by the exposed surfaces of the ends of the brine cells amounting on an average to 1600 square feet at a mean temperature of about 20° F.

The refrigerator is one of Pontifex and Wood's ammonia machines with a nominal ice-making capacity of 9 tons per 24 hours.

During the months of June, July, and August, though the apparatus was in pretty full work, there still remained many details to be adjusted, but the general cooling results were very satisfactory. For the week ending the 25th July the following were the readings of thermometers inside and outside the house:—

	Outside Thermometer on North Wall.	Inside Thermometer 5ft. from floor in centre of House.
20 July, at 1 p.m.	62°	31°·5
21 " "	60	30·5
22 " "	62	30·0
23 " "	72	32·0
24 " "	74	32·0
25 " "	79	31

During this week one of the ammonia pumps was off for repair, so that the refrigerator was worked at about one half of its full power. As a rule it is not necessary to work the refrigerator on Sunday as the temperature inside the house only rises about 1° between Saturday and Monday. As the details of the plant were gradually perfected the results showed a corresponding improvement. For the week ending are the readings of the thermometer inside and outside:—

	Outside Thermometer on North Wall.	Inside Thermometer, 5ft. from floor in centre of House.
14 September, at 3 p.m.	56°	28°
15 " "	62	29
16 " "	58	29
17 " "	58	29
18 " "	60	28
19 " "	56	28

The refrigerator was doing about half work during this week, as we have found that 27° or 28° is a sufficiently low temperature for the air of the house—at lower temperatures the oil filters sluggishly.

The lowest temperature ever observed on the inside thermometer was 26°, and the greatest difference between the outside and inside thermometers was 45°. Of course, the temperatures on the south and west walls, in the direct rays of the sun, must often have reached 90° to 100°, giving a difference of 65° between the outside and inside.

It was at first feared that the workmen might suffer from the sharp contrast between a summer temperature outside and a winter temperature inside the house; but, apart from an occasional cold, no bad effects have been traced to this cause.

Cooling of the Oil and Paraffin.

In connection with my former paper, a diagram was exhibited (*Journal*, iv. 324), on which were laid down the curves of cooling of a mass of oil and paraffin as determined in an experimental cell of the standard width, 12 inches. These curves showed that, with air of a mean temperature of 20° F. as the cooling medium, the mass of paraffin in the cell was reduced to a mean temperature of 30° in ninety-six hours. In practical working, it is found, with brine at 10° F. as the cooling medium, that the mass of oil and paraffin in a cell 16′ × 8′ 6″ × 12″ wide is reduced from 100° F. to a mean temperature of 24° in from seventy to seventy-five hours. At this rate of cooling the crystallisation of the paraffin is very satisfactory.

Manipulation of the Cooled Oil.

No difficulty has been experienced in emptying the cells by means of the screw at the bottom of each. When the screw is rotated the block or slab of cooled oil invariably slips down as its base is sheared away, no manual or other assistance being required.

Owing to the great stiffness of the cooled paraffin, even after it has been mashed by the screw and delivered through the valve at the end of the cell, considerable difficulty was experienced in getting the filter press-pumps to draw and force the mass into the filter frames. After contending with this difficulty for some weeks, it was eventually found necessary to abandon the close collecting pipes from the cells to the pumps, replacing them by open wooden troughs. Since this alteration has been carried out the working has fallen into the regular system for which it was planned, so that one of the four blocks (of ten cells each) is emptied and refilled every day.

Owing to the perfect crystallisation of the paraffin, the separation of the oil is much facilitated, so that filter presses which, under the old system of "drum cooling," took ten, eighteen, and even twenty-four hours to fill up, are now filled regularly every three hours.

In the following table the old and new systems of paraffin cooling are contrasted in the records of working under each when the conditions of outside temperature were identical :—

	1884. Old System.		1885. New System.
Outside Thermometer on north wall ..	60° F.	....	60° F.
Inside „ 5 feet from floor	56	....	28
Oil from coolers	18	....	24
„ filter presses	41	....	25

The old system is imperfect in two important particulars ; in the first place, the too-rapid cooling produces a paraffin which will not separate freely from the oil, and in the second place, the cooled oil itself is the only medium by which the temperature of the apparatus is reduced and kept down ; consequently, the oil and paraffin has to be cooled to a temperature much below that at which the filtration actually takes place.

The figures given above show that under the old system the oil was cooled 26° below the temperature at which filtration actually took place, while, under the new system, the difference is only 1°. The above figures are not put forward to show the best results obtainable under the old system, but merely to illustrate the distinct principles on which the systems are worked.

Quantity and Quality of the Paraffin.

The gain of paraffin by the new system is about 25 per cent., so that a crude oil, which yielded under the old system 10 per cent., now yields 12½ per cent. of paraffin. This on the present through put at Oakbank Works is equal to 180 tons of paraffin a year, valued at £4500.

This gain of paraffin is not alone due to the low temperature of extraction, but to a considerable extent results from other modification of the refining process which have been rendered possible by the system of slow cooling and crystallisation.

The average melting point of the paraffin scale extracted by the new process is 112° F., or a fall of about 6° as compared with the old process. The scale on refining gives :—

74 per cent. of wax, melting point 120° F.
11 „ „ „ scale, „ „ 90° F.
15 „ „ „ oil and unrecovered soft scale.

It is now possible to produce commercially-soft paraffin of any melting point, from 80° F. and upwards. Hitherto most of the low melting paraffin made in Scotland has been used for dipping wooden matches, but the quantity at present being produced is likely to be far in excess of the requirement of the match trade. It is much to be desired that new outlets should be found for it, and it is to be hoped that one good effect of the present low price (1d. to 1½d. per pound) will be to extend its consumption. It is said that soft paraffin is being used to adulterate tallow ; fortunately, as an adulterant, it will be detected with great ease, but it seems not improbable that the mixture of soft paraffin with tallow and lard may be found to improve both for many purposes.

Working Costs.

Twelve men at an average wage of 3s. 6d. per day can turn out 26 tons of paraffin scale per week, equal to a cost of 10s. 6d. per ton. To this must be added the wages of enginemen and foreman, making a total of say, 15s. per ton. When the output can be increased to 50 tons per week, the cost for labour will be reduced to about 12s. per ton.

Owing to the small size of the refrigerator for the very large amount of actual cooling done, the cost under this head is necessarily very moderate.

An important item of paraffin house expenditure is for sheeting and filter cloth. Trustworthy statistics of the saving under this head are not obtainable yet, but there is no doubt that the saving will be considerable, as the well-crystallised paraffin separates freely both in filter-presses and hydraulic-presses.

DISCUSSION.

Mr. COLEMAN said : This paper by Mr. Beilby is a continuation of a paper which he read last session, and in which he described a method of cooling and filtering and pressing paraffin oil in chambers which are insulated. As to the mechanical arrangements which Mr. Beilby has adopted for the transference of the crystals of cooled paraffin to the filter presses, it shows, as I have already said, great ingenuity. This kind of screw prevents the handling of the material, which is so prejudicial to bringing up the crystals. They are more easily transferred without being crushed or broken, and consequently, the produce of wax is increased. In reference to the process itself, being conducted within insulated walls, it is no doubt a most excellent thing to do, and it surprises me that the paraffin oil people have not had the sense to adopt some such method earlier. It must be obvious that to cool a substance down near freezing point, and then filter and press it in a warm atmosphere, is a most ridiculous proceeding, and in many circumstances the cooling is a most expensive operation. One of our townsmen—Mr. W. Walls—is noted for his sperm oil. He had five years ago another machine which he used for cooling sperm oil, and it struck him that there must be immense waste in the oil being manipulated, after cooling, in the ordinary warm air of his workshop. He consulted me as to whether I could not design a cold chamber, in which the process might be carried on. I designed one, and since that time his production of spermaceti has been considerably increased, and the quality of his oil has been improved.

Professor MILLS said : In common with many others who have visited a paraffin oil works, I have been very much struck with the absence of air-cooling in the department where the scale is frozen and pressed. I am very glad that Mr. Beilby has not only introduced it, but succeeded in making it pay. The increase of 2½ per cent. must be chiefly in soft scale. Paraffin of this kind would have been, years ago, exceedingly valuable to the late Dr. Stenhouse, with whom I was associated in his early experiments on the waterproofing of cloth. At that time we required a paraffin of extreme qualities, some very hard and some very soft. Paraffin similar to this specimen would have suited us admirably, and would have made a capital waterproofing agent. As we had not paraffin of this description we were obliged to take hard paraffin and mix it with olive oil, so that when the waterproofing agent was introduced into the tissue—for example, boot leather—the crystals of scale would not produce friction. It struck me that this particular paraffin, this new soft scale, would have been the very thing for waterproofing cloth, inasmuch as it would yield to every movement of the wearer. I am sure this contribution to the technology of the subject is one of material interest and importance, and I hope Mr. Beilby will succeed in increasing the sale of soft scale.

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HIGH TEMPERATURE THERMOMETERS.

BY JAMES MURRIE.

Since reading the paper on a new series of thermometers before this section of the Society last session, a few modifications have been made for the purpose of indicating very high temperatures, a description of which may prove interesting.

The principles embodied in the construction of the apparatus have been already explained (*Journal*, iv. 45, 189). The essential feature consists in the raising of the boiling point of a liquid by pressure, the liquid being confined in an expansion chamber, which forms one end of a hollow tube, the opposite end of the hollow tube being practically sealed by means of a pressure indicator. On taking indications of temperature, the expansion chamber is subjected to the action of the heat; the confined fluid in consequence expands, and in the act of expanding tends to compress the liquids placed in the hollow stem intermediate to the expansion chamber and the pressure indicator; the indicator being graduated to show the corresponding temperature.

For indicating low temperatures, a fluid of comparative volatility may be employed. For temperature above 400° Fahr., mercury has been found suitable, the upward limit of an instrument for continuously indicating the temperatures being 1700° Fahr. Above this point the metal becomes too plastic and porous to contain the vapour.

For continuously indicating temperatures above 1700° Fahr., the apparatus is not adapted, although it can be modified to enable indications being taken at intervals, the maximum range being about 4000° Fahr. The expansion chamber is enveloped in a conducting body, and attached to, or forming part of, the indicator is a time recorder. On the expansion chamber with its coating being brought in contact with the heating body, the mercury in the expansion chamber is slowly heated, depending more particularly on the intensity of the heat surrounding said casing. The indicator registers the degree of heat, and a chronograph records the time taken to raise the temperature of the mercury between two predetermined points. It is, of course, necessary to withdraw the instrument from the action of the heat before the temperature of the mercury is sufficiently raised to create a pressure which would destroy the instrument.

For very high temperatures, more particularly when the heated body is in a gaseous form, the difficulty is in obtaining a sufficient volume of gas at a nearly uniform temperature. Above 3000° Fahr., the temperature is rarely constant for any period at a fixed point, and in consequence the indications obtained by means of the apparatus cannot be taken at the maximum temperature existing in the furnace or oven. When the heated body is in a liquid form, and the chamber completely immersed in the metal, the temperature can be better determined.

For temperatures under 1700° F., the main difficulty experienced has been in preventing leakage of the confined fluids, a very slight leakage having the effect of emptying the chambers of their contents.

This difficulty, however, might possibly be lessened, if not overcome, with the result that the duration of immersion would not materially affect the accuracy of the register of temperature. In stating this I understand that a London scientist has given expression to different views from those I have embodied; but I have seen nothing to shake my convictions as to the accuracy of my conclusions in the matter.

In answer to various questions, Mr. Murrie said: The only liquefied gas which has been tried in the thermometer is liquid carbonic anhydride. It is at present very difficult to reduce hydrogen, nitrogen and other gases, to the liquid state; but I am at present engaged in a new process for liquefying gases rapidly and with facility, which at present I am not at liberty to describe. In the case where liquefied gases are employed, the inner chamber is, by means of a tube with a stop cock, connected with a generator in which the gas is liquefied, and from which the chamber is charged. The highest temperature at which these liquefied gases can be used depends on the pressure of the saturated vapour. Some of these vapours have so great a pressure at ordinary temperatures that it would be impossible to retain them in such an apparatus; but liquefied carbonic anhydride, for example, could be kept in the thermometer, say, to the temperature of boiling water. To take the temperature of a small blast furnace, it would have to be so constructed that the chamber of the thermometer, when inserted, would be surrounded by some comparatively non-conducting material. Thus, the mercury chamber would be surrounded with clay, through which heat passes very slowly. The mercury becomes heated at a corresponding rate, and the time taken to heat the mercury, say, to 1500° F., is noted on a chronometer, and the temperature calculated from this.

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Notices of papers and communications for the meetings to be sent to the Local Secretary.

The opening Meeting of the First Session of this Section was held in the Laboratory of the Basford Station of the Nottingham Corporation Gas Committee, on Saturday, November 14, 1885.

ON THE ANALYSIS OF GAS COAL.

BY LEWIS T. WRIGHT, F.C.S., ASSOC. MEMB. I.C.E.

COAL analysis as understood by a gas-maker refers principally to two important factors—viz., the volume of gas a coal is capable of producing, and its luminosity under the conditions of his manufacture. Of course there are several other points, such as quantity and quality of coke, tar and ammoniacal liquor, sulphur, ash, etc., which must engage his attention; but the first question he naturally asks in reference to a new coal is, What quantity of gas will it produce, and of what candle-power? A correct knowledge of the capabilities of a coal is of double importance to us as gas manufacturers. In the first place we can assign to it a pecuniary value, and in the second place we are, by a knowledge of the various coals we employ, enabled to better supervise and control our operations, and determine our manufacturing losses—if any.

In the present paper I propose to consider the various methods of analysis, and, to some extent, by comparison, to discuss their relative merits, and finally to compare the results of the most approved method with actual manufacturing experience.

For the examination of a coal from a gas-maker's particular point of view, there are several means—(1.) To make a proximate analysis, and thereby discover how much moisture, volatile matter, coke, ash, and sulphur the coal contains. (2.) To make a practical analysis of the coal by distilling it (*a*) either in a small iron retort with other necessary apparatus for collecting the gas and measuring its volume, etc.; or (*b*) in a setting of clay retorts as used in modern gas manufacture, with other necessary plant for exhausting, condensing, scrubbing, purifying, measuring the gas, and collecting and measuring the liquid products and coke, just on the same lines as we operate in gas manufacture. There is another method—viz., elementary analysis, perhaps of more interest to a chemist than to practical people, but which should not be neglected if a complete knowledge of the coal is required.

Elementary Analysis I will not enter into, as it is fully treated on in the best text books, such as Fresenius', etc., but merely mention that the oxidising material preferred is lead chromate.

Proximate Analysis.—Proximate analysis may be said to consist of the following determinations:—Moisture lost at some given temperature; volatile matter; fixed carbon; ash; sulphur.

The first determination is that of moisture, and at once plunges us into doubt. At what temperature shall the sample be dried? It being known that coal does not lose all its moisture at 100° C., some recommend drying at higher temperatures than that of boiling water. Hinrichs says, put down as moisture the loss in weight of finely-pulverised coal after drying for one hour at a temperature between 105° and 110° C. Fresenius recommends 110° C.; but coal dried at 110° C. will lose more water if further dried at 150° C., and so on. So it does not appear that a temperature of 110° C. has any special recommendation in respect of obtaining all the moisture. Since 100° C. is a temperature easily secured and maintained constant by means of a water-bath, I have adopted it as a standard.

The usually adopted rule of estimating moisture by loss of weight in drying at a given temperature, is to proceed with the drying until the weight of the substance becomes constant. This procedure is not free from error. In the first place, the loss of weight by drying at about 100° C. for any given time does not equal the actual amount of water evolved. In the second place, if most samples of coal be examined by the drying method of weighing at certain intervals of time, it will be found that after a time the samples not only cease to lose weight, but actually gain. This gain has been noticed by different observers. Hinrichs attributes the increase to the slow oxidation of some matters in the coal (pyrites, etc.); but this is a subject deserving of independent investigation, and will perhaps be found to be due to the absorption of gases into the pores of the coals left vacant by the expelled moisture. It is reasonable to suppose that the increase of weight which only exhibits itself after a large portion of the moisture has been got rid of, has really been taking place during the whole period of the drying process. In that case the water actually lost will be more than is shown by loss in weight, and this I have always found. Some experiments with coals of different character will exhibit this:—

Scotch splint, finely powdered, dried 1¼ hours in an atmosphere of coal gas—			
Moisture lost at 100° C., weighed as water in a drying-tube	(*a*)	10·22	per cent.
Moisture estimated by loss of weight in drying the coal at 100° C.	(*b*)	9·66	,,
Another experiment given for the two methods	(*a*)	10·31	,,
	(*b*)	9·76	,,
Durham gas-coal	(*a*)	1·79	,,
	(*b*)	1·59	,,
Welsh cannel	(*a*)	4·74	,,
	(*b*)	4·36	,,

The difference between the two methods may be taken to represent the increase in weight during the drying process, and may be tabulated as follows:—

Scotch splint, first experiment	0·56	per cent.
Do. do., second experiment	0·55	,,
Welsh cannel	0·38	,,
Durham gas-coal	0·20	,,

From which it might be concluded that the increase in weight varies in some direct proportion with the moisture.

It will therefore be seen that the ordinary method of determining the moisture in coal by loss of weight in drying is not satisfactory, and that where accuracy is required, the water must be actually weighed as such. In any case I would suggest 100° C. as the drying temperature. The determination of volatile matter will be found even less satisfactory than the foregoing determination, until some standard method, however arbitrary it may be, has been adopted by all operators. Professor Hinrichs states that the total volatile matter of coal is determined with accuracy (1 milligram on 1grm. of coal) by taking 1 to 2grm. of undried pulverised coal, heating for 3½ minutes over a Bunsen burner (bright red heat), and then immediately, without cooling, for the same length of time over a blast gas-lamp (white heat); and he has published some experiments in support of his conclusion, from which the following series is quoted:—

Weight of Coal.		Volatile Matter. Per cent.		Deviation.
1·910grm.	....	49·58	....	−0·18
1·117 ,,	....	49·87	....	+0·11
1·031 ,,	....	49·85	....	+0·09
Average......		49·76		
Greatest difference		....		0·29

Adopting the above method of Hinrichs, I obtained, by exercising extreme care, the following results with two different samples of undried powdered coal—

DEAN'S PRIMROSE GAS-COAL.

(This coal has been exposed to the atmosphere in a finely powdered state for two years.)

Weight of Coal.	Volatile Matter. Per Cent.	Deviation.
2·018grm.	31·22	−0·28
2·014 „	31·62	+0·12
2·009 „	31·66	+0·16

Average...... 31·50

Greatest difference .... 0·44

SILKSTONE GAS-COAL.

Weight of Coal.	Volatile Matter. Per Cent.	Deviation.
2·059grm.	41·57	+0·28
2·022 „	40·90	−0·39
2·005 „	41·39	+0·10

Average...... 41·29

Greatest difference .... 0·67

The above experiments are not selected, but were consecutive, and though not so promising as those published by Hinrichs, they are fairly comparable.

The following experiments were made by myself. The method of operation was to weigh the coal in a thin platinum crucible without the cover, which was only used during the heating and cooling in the desiccator. The reason for not weighing the cover was that it becomes coated on the inside with a thick deposit of soot, which clearly belongs to the volatile matter. The inside of the crucible also acquires a thin carbonaceous deposit (in some of the experiments allowed for); but the error due to this deposit is small compared with those due to different systems of heating. However, it always tends to make the volatile matter too low. In these experiments volatile matter includes all moisture.

1. Experiments with Scotch splint coal (moisture 10·26 per cent.). Gas flame 600mm. long, 100mm. diameter; coal in small lumps; flame applied until all appearances of gases ceased issuing from under lid of crucible :—

Weight of Coal.	Volatile Matter. Per Cent.	Deviation.
4·768grm.	41·69	−1·11
4·583 „	43·16	+0·36
4·472 „	43·96	+1·16
3·011 „	42·70	−0·10

Average...... 42·80

Greatest difference .... 2·27

2. Coal finally powdered, all other conditions as in above series :—

Weight of Coal.	Carbon Deposit on Crucible.	Volatile Matter. Per Cent.	Volatile Matter corrected for Carbon. Per Cent.	Time. m.	Time. s.
1·110grm.	0·008	43·25	43·60	1	30
1·551 „	—	41·61	—	1	45
2·672 „	0·001	43·48	43·01	2	35
3·600 „	—	43·27	43·55	3	30
3·878 „	—	42·13	—	3	45
4·939 „	—	42·70	42·80	4	30

Average ...... 43·21

Greatest difference .... 2·18

This method of heating till all appearances of gases cease issuing from the crucible, has not been found satisfactory. The experiments with powdered coal appear least so; but against this view might be urged the greater difference in the weights of coal taken for each experiment.

3. Welsh cannel (moisture 4·74 per cent.). Gas-flame as before, but heating continued for a constant period of 90 seconds :—

Weight of Coal.	Volatile Matter. Per Cent.	Deviation.
2·005grm.	49·17	+0·39
2·080 „	48·79	+0·01
2·072 „	48·26	−0·52
2·021 „	48·91	+0·13

Average ...... 48·78

Greatest difference .... 0·91

4. Silkstone gas coal (moisture 7·75 per cent.). Same conditions as in series No. 2, with the exception of varying power of gas-flame—

Volatile Matter. Per cent.	Length of Gas-Flame.
41·35	600mm.
40·23	300 „
39·93	200 „
39·48	100 „

These experiments show fairly well the differences due to the power of the gas-flame.

5. A method was tried of heating until the gases ceased, and then heating for a further period of 1 minute. Gas-flame 600mm. long, 100mm. diameter; coal powdered :—

Weight of Coal.	Volatile Matter. Per Cent.	Deviation.	Time Gases Ceased. m.	Time Gases Ceased. s.	Total Time. m.	Total Time. s.
1·187grm.	40·66	+0·01	1	0	2	0
1·463 „	40·53	−0·12	1	15	2	15
3·142 „	40·70	+0·05	2	30	3	30
4·150 „	40·70	+0·05	3	20	4	20

Average .... 40·65

Greatest difference .... 0·17

These results were thought to be sufficiently good, and the method was afterwards tried with various samples of coal.

6. Dean's primrose gas coal (fresh). Conditions the same as in series No. 5 :—

Weight of Coal.	Volatile Matter. Percent.	Deviation.	Time Gases ceased. m.	Time Gases ceased. s.	Total Time. m.	Total Time. s.
2·024grm.	33·59	+ 0·01	1	10	2	40
2·066 „	33·51	− 0·06	1	50	2	50
2·139 „	33·66	+ 0·06	1	55	2	55

Average .... 33·60

Greatest difference ...... 0·12

7. The same powdered sample of coal was tried after two years' exposure to air. Gas-flame 600mm. long, 100mm. diameter :—

Weight of Coal.	Volatile Matter. Per Cent.	Deviation.	Time Gases ceased. m.	Time Gases ceased. s.	Total Time. m.	Total Time. s.
(1) 2·030grm.	30·88	+ 0·02	2	10	2	40
(2) 2·113 „	31·01	+ 0·15	2	15	2	50
(3) 2·005 „	30·67	− 0·19	2	40	3	20

Average .... 30·86

Greatest difference ...... 0·37

The time required to expel the gases in these three experiments discloses an apparent anomaly which requires explanation. In all experiments it had been the practice to maintain the coal-dust as an even layer on the bottom of the crucible, and to keep the crucible in an upright position during the heating. The ring which was used in this series (7) being small, caused the crucible to cant over, and shifted the coal-dust into a little heap in the corner. This heap being thicker in the centre than it would have been if spread evenly over the bottom of the crucible, was less favourably disposed for quick heating. This explanation of the apparent anomaly was verified by an examination of the coke residue. Trifling as this point may appear, it should have attention in accurate experiments.

8 West Leverson coal. Same conditions as in No. 6 :—

Weight of Coal.	Volatile Matter. Per Cent.	Time Gases ceased. m.	Time Gases ceased. s.	Total Time. m.	Total Time. s.
2·084grm.	32·39	1	50	2	50
2·113 „	32·18	2	10	3	10

Average ...... 32·28

Difference .... 0·21

9. Leverson coal. Same conditions as in No. 8 :—

Weight of Coal.	Volatile Matter. Per Cent.	Time Gases ceased. m.	Time Gases ceased. s.	Total Time. m.	Total Time. s.
2·153grm.	33·12	1	55	2	55
2·065 „	33·02	1	50	2	50

Average ...... 33·07

Difference .... 0·1

10. Felling Main coal. Same conditions as in No. 8:—

Weight of Coal.	Volatile Matter. Per Cent.	Time Gases ceased. m.	s.	Total Time. m.	s.
2·205grm.	32·88	2	0	3	0
2·211 "	31·80	2	0	3	0
Average	32·31				

Difference .... 1·08

11. Australian shale. Same conditions as in No. 8:—

Weight of Coal.	Volatile Matter. Per Cent.	Time Gases ceased. m.	s.	Total Time. m.	s.
2·009grm.	78·69	1	20	2	20
2·017 "	78·58	1	25	2	25
2·019 "	78·55	1	26	2	26
Average	78·61				

Greatest Difference .... 0·11

Series 5, 6, 8, 9, 10, and 11 were all conducted in the same manner, and with the exception of No. 10 were very satisfactory. For want of a better method I have adopted the following:—Take about 2grm. of finely pulverised coal, and let it form an even layer on the bottom of a thin platinum crucible. Weigh without cover, place the crucible (with cover on) in an upright position on an iron ring, round which has been wound sufficient platinum wire to prevent contact between the iron and the platinum crucible. Then apply a powerful gas-flame (to secure even results endeavour to keep the same length of flame in all experiments). Note when the gases cease issuing from under the lid; allow one minute further heating; remove the gas-flame, place the crucible and cover in a desiccator for about five minutes to cool, and then weigh without cover as soon as possible.

It is extremely difficult to get fair samples of cannel coal, there often being several layers within a few inches of widely different quality. The following case is quoted:—A piece of curly cannel was taken, 6 inches deep, and on examination it was found that it consisted of various thin layers without any very sharp definition, but gradually merging together. The different appearances of the layers and the volatile matter are as follows:—

Very bright black curly	78·8 per cent. 76·0 " 79·1 "
Shiny curly	76·3 "
Dull curly	69·5 "
Smooth	61·7 "

The determination of ash is one that admits of great accuracy, if performed with due care. The very common mode of operating in platinum boats or dishes, placed on an open flame, is subject to a serious error, due to draughts carrying away some of the very fine and light particles of ash. Cannel and easily incinerated cokes may be treated in a deep platinum crucible; but very refractory cokes and such substances as gas carbon require a different method.

Weigh about 2grm. of the coal or coke in a piece of platinum foil, rolled so as to admit of its easy passage into a piece of combustion-tubing of about 1in. diameter, and heat to redness, whilst a very gentle current of air is being drawn through the combustion-tube by means of an aspirator. Working in this way, a beautifully clean ash can be obtained in a very short time; but it is necessary to guard against using a heat that would cause the foil to fuse to the glass. It is well to know that coke absorbs moisture very rapidly. For this reason it is best to take a separate portion of coal for the ash determinations, and not to weigh a portion of the coke obtained in the volatile matter determination. The ash should be saved for the further determination of the sulphuric acid therein.

For the determination of total sulphur there have been suggested innumerable methods, such as fusion with alkali and alkaline carbonates and nitre; boiling with hydrochloric or nitric acid and chlorate of potash; aqua regia; fuming nitric acid under pressure in sealed tubes; combustion with calcic hydrate, etc.; but, in my opinion, by far the best and simplest method is that of Nakamura, which is thus described:—

> It consists in heating the coal below a red heat in contact with alkaline carbonates, by which the coal, whether bituminous or not, rapidly undergoes, without evolution of smoke, complete atmospheric oxidation in a manner hardly to have been expected. The details of the method are as follow:—Take 3 or 4 parts of the mixed alkali carbonates, or of sodium carbonate, to 1 part of coal in very fine powder. Intimately mix in a large platinum dish or crucible with a dry glass rod, and heat the mixture in the dish or crucible loosely covered, at first so gently as not to volatilise the hydrocarbons—that is, so that no smell, or only a very faint aromatic odour, is observable—a matter much more easy of execution than might be supposed. Use an Argand spirit-lamp instead of a Bunsen burner, to avoid possible absorption of sulphur from the flame of coal gas. Keep at a low temperature for some time; then raise the heat by slow degrees (without letting it reach that of visible redness) until the surface, which is at first of a dark grey colour, becomes only faintly grey. No smoke or odorous gases should escape during the whole of the oxidation. When the surface becomes only faintly grey, raise the temperature to a faint red heat, and keep it stationary for about 40 to 60 minutes, at the end of which time the mass will become almost perfectly white, or reddish if the coal contains iron, from the complete combustion of the coal. The mass is then treated with water, filtered, and the sulphate is determined in the filtrate as usual after acidification. To succeed in the complete oxidation of coal by this method, it is most important to pulverise it as fine as possible. The best plan is to sift it through cotton cloth. . . . Stirring the mixture during the oxidation impedes the process instead of hastening it, probably by closing the passages left by the unconsumed coal.

If the above directions are well carried out, the method leaves nothing to be desired.

It is necessary to distinguish between the sulphur which goes over in the volatile matter, the sulphur left in the coke (which, however, can be converted into sulphurous acid by combustion), and the sulphur which is finally left in the ash combined as sulphate. For this purpose three determinations are required—

1. Total sulphur by Nakamura's method.
2. Sulphur which is converted into sulphurous acid by combustion of the coke in air. This can be done by roasting a quantity of coke representing a known quantity of the coal on platinum foil in a glass combustion-tube, and aspirating the gaseous products of combustion through a solution of iodine or bromine.
3. Determination of sulphur in ash. This can be done in most cases by boiling the ash with a little hydrochloric acid, filtering and determining the sulphuric acid in the filtrate in the usual manner, or by fusion with alkaline carbonates.

PROXIMATE ANALYSIS OF AUSTRALIAN SHALE
(sp. gr. 1040·1).

Water lost at 100°C.	0·11	—
Volatile matter	77·69	Corrected for sulphur.
Fixed carbon	5·56	" "
Ash	15·83	" "
Sulphur in volatile matter	0·18	0·1518
" fixed carbon		0·0121
" ash		0·0137
	100·00	

The practical method partakes of two forms—a partial imitation of the process of gas-making on a small laboratory scale. This method has the advantage of being rapid and fully under control, and being effected in small compact closed apparatus, can be conducted without any of the losses of a large manufacture, and gives us as a result an ideal that cannot perhaps be entirely reached in a large-scale manufacture; yet tells at once what is in the coal, and what we must try to secure in our works. The other practical method is by conducting a gas manufacture just as in actual practice, with large plant such as is in vogue.

Laboratory Practical Analysis.—The apparatus consists of a small and accurate weighing-machine, provided with an arm scaled to hundredths of a pound and capable of easily turning to this quantity. A cast-iron retort 3ft. 6in. long, of D section 5 × 3½, closed at one end, set horizontally over a furnace, with flues so arranged as to admit of its being evenly heated to any required reasonable temperature. A length of about four inches of the open end is left clear of the brick-work, to provide for the mouthpiece lid and ascension pipe. A chimney, fitted with a damper, is arranged in connection with the flues, to create a draught capable of regulation. The open end of the retort protruding from the brickwork has fitted to it a lid or door hinged on the left hand side, fitted with a cross-bar, which engages in a catch in the right hand side of the mouth of the retort, and a screw arrangement for pressing the lid to the end of the retort so as to hermetically close it, both mouthpiece end and door being properly faced.

On the top of the protruding end of the retort, and at right angles to it, a 2in. pipe, provided with a cock, is screwed in to form the ascension pipe and valve for closing it off from the other apparatus. From the ascension pipe, a rather smaller one (say 1½in.) is carried at a height of about 7ft. from the floor, so as to allow of head-room, and is turned down again to connect to a condenser conveniently placed against a wall, so as to be out of the way. This condenser is made up of about 60ft. run of a ¾in. pipe, provided with cocks to run off the tar and liquor which collects. These pipes are placed in a water tank. The inlet to the condenser is fitted with a cock, and the outlet to two small cast-iron purifiers, 12in. × 12in. × 6in., having cast-iron lids dropping into water-tubes cast on the sides of the purifiers. The weight of these lids is sufficient to hold them in their places without fastening. The inlet to these purifiers is at the bottom; and about 2in. above, on small feet, is a wooden grid to hold the purifying material (moist slaked lime), a small tube-like arrangement is cast on the inside leading from the top to the bottom, down which the gas passes, after having traversed upwards through four inches of lime, and then by a piece of gas-piping to the inlet of the second purifier, which is similar in all respects to the first. To the outlet of the second purifier is a pipe leading to the inlet of the gas-holder. This gas-holder has a capacity of about 12 to 13 cubic feet, and carries on it a vertical brass scale accurately divided into cubic feet, tenths and hundredths. The holder works in a cast-iron tank filled with water, and is fitted with rollers, top and bottom, for guiding it in its passage up and down. The rollers at the top are two in number, and work against a guide-rail placed on two upright pillars mounted on the top of the tank, opposite each other. These pillars are connected by a cross head, whereon is mounted a wheel, over which travels a strong wire cord, connected at one end to the centre of the crown of the gas-holder, and at the other to an iron rod and circular plate, to carry the counterpoise weights—slotted circular discs of cast-iron. A pipe which passes up through the tank above the water-line serves as inlet and outlet by an arrangement of 3 cocks on each leg of a T piece placed outside. The gas can be caused to either enter the holder from the purifier, or go out from it to the photometer, or go from the purifier to the photometer direct, without communication with the holder. To compensate for the increasing weight of the gas-holder when rising out of the water, a special counterpoise is arranged, which need not be specially described. The crown of the holder is flat, excepting at the centre, where there is a sort of cap just over the ascending pipe, which is a few inches above the crown when the holder is down. The object of this is to enable the water to nearly fill the crown of the holder without flowing over the top of the ascending pipe, and thus stopping the flow of gas. As even then some water might by accident get over, a small plug or tap is placed in the pipe outside the tank to admit of the removal of such water. A thermometer and water-gauge are mounted with the holder to indicate the pressure and temperature. When gas is entering the holder from the retort during an experiment, the holder is counterpoised so that the the water-gauge is level. When it is desired to remove gas from the holder, as in testing it by the photometer, weights are taken off the counterpoise rod to produce any convenient pressure. Before commencing a trial, some of the coal to be tried is placed in the hot retort, and the gas allowed to enter the holder. This gas is blown back, and burnt at the mouth of the retort, the cock in ascension-pipe then closed, and the residue in the retort carefully raked out. Thus the whole apparatus is charged with gas of similar nature to that to be examined. Then the cock leading from the holder to the photometer, and the cock on the ascension-pipe being closed, and all other communication open, a quantity of 2·24lb. of the coal, or 1000th part of a ton, is introduced by means of a suitable long iron scoop into the hot retort and turned over. Three simultaneous operations have now to be performed. The scoop has to be drawn rapidly out with the right hand, and dropped; the left hand closes and screws up the lid or door of the retort, and the right hand being free, opens the cock on the ascension-pipe. With practice, this combined operation can be so performed that no gas escapes. The gas then passes through the condenser and purifier into the holder, and in a quarter to half-an-hour, according to the heat and nature of the coal, all gas is extracted. To ascertain when this is complete, the gas-holder scale can be watched till it remains stationary. But I prefer also placing a water-gauge on the pipe leading to the condenser, and by shutting the tap on the inlet before mentioned, observing whether there is any movement of the gauge, which there would be if any gas were coming from the retort. This point satisfactorily settled, the cock on ascension-pipe can be closed, the retort door opened, and the coke raked out into an iron pan, and carefully weighed. The tap on the inlet gas-holder being closed, and the one leading to the photometer open, the illuminating power can be determined according to usual methods. It will be observed that this apparatus, as above-described, only provides for three results. (1.) The quantity of gas. (2.) Its illuminating power. (3.) The weight of residual coke. But by means of a conveniently-designed condenser, I have found it possible to obtain a fairly accurate idea of the quantities of tar and gas-liquor yielded; but, as no scrubbing operation is arranged for, the total quantity of ammonia is not obtainable. These two quantities—the tar and ammonia—are generally neglected on this form of testing apparatus; the chief desideratum being a rapid means of finding the gas-making value of the coal. The volume of gas, if collected in the holder, is of course corrected for temperature and atmospheric pressure to the standards of 60° F. and 30° Bar. by means of the table on pages 660 and 661.

This apparatus, properly and intelligently handled, gives results of the utmost value to the gas-maker;

CORPORATION OF NOTTINGHAM.

GAS DEPARTMENT.

TABLE TO FACILITATE THE CORRECTION OF THE VOLUME OF GAS AT DIFFERENT TEMPERATURES AND UNDER DIFFERENT ATMOSPHERIC PRESSURES.

	THERMOMETER.																											
Barometer.	40°	42°	44°	46°	48°	50°	52°	54°	56°	58°	**60°**	62°	64°	66°	68°	70°	72°	74°	76°	78°	80°	82°	84°	86°	88°	90°	92°	94°
28·0	·979	·974	·970	·965	·960	·956	·951	·946	·942	·937	·932	·927	·922	·917	·912	·907	·902	·897	·892	·887	·881	·875	·870	·865	·859	·853	·847	·841
28·1	·983	·978	·973	·969	·964	·959	·955	·951	·945	·941	·936	·930	·926	·921	·916	·911	·905	·900	·895	·890	·884	·879	·873	·868	·862	·856	·850	·844
28·2	·986	·981	·977	·972	·967	·963	·958	·953	·949	·944	·939	·934	·929	·924	·919	·914	·909	·904	·898	·893	·887	·882	·876	·871	·865	·859	·853	·847
28·3	·990	·985	·980	·976	·971	·966	·961	·957	·952	·947	·942	·937	·932	·928	·922	·917	·912	·907	·902	·896	·891	·885	·880	·875	·868	·862	·856	·850
28·4	·993	·988	·984	·979	·974	·970	·965	·960	·955	·951	·946	·941	·936	·931	·926	·921	·915	·910	·905	·900	·894	·888	·883	·878	·872	·866	·860	·854
28·5	·997	·992	·987	·983	·978	·973	·968	·964	·959	·954	·949	·944	·939	·934	·929	·924	·919	·914	·908	·903	·897	·892	·886	·881	·875	·869	·863	·857
28·6	1·001	·995	·991	·986	·981	·977	·972	·967	·962	·958	·953	·947	·943	·938	·932	·927	·922	·917	·912	·906	·901	·895	·889	·884	·878	·872	·866	·860
28·7	1·004	·999	·994	·990	·985	·980	·975	·970	·966	·961	·956	·951	·946	·941	·936	·931	·925	·920	·915	·909	·904	·898	·893	·887	·881	·875	·869	·863
28·8	1·007	1·003	·998	·993	·988	·984	·979	·974	·969	·964	·959	·954	·949	·944	·939	·934	·929	·924	·918	·913	·907	·901	·896	·890	·884	·878	·872	·866
28·9	1·011	1·006	1·001	·997	·992	·987	·982	·977	·973	·968	·963	·958	·953	·948	·942	·937	·932	·927	·921	·916	·910	·905	·899	·893	·887	·881	·875	·869
29·0	1·014	1·010	1·005	1·000	·995	·990	·986	·981	·976	·971	·966	·961	·956	·951	·946	·941	·935	·930	·925	·919	·914	·908	·903	·897	·891	·885	·879	·873
29·1	1·018	1·013	1·008	1·004	·999	·994	·989	·984	·979	·975	·969	·964	·959	·954	·949	·944	·939	·933	·928	·923	·917	·911	·906	·900	·894	·888	·882	·876
29·2	1·021	1·017	1·012	1·007	1·002	·997	·992	·988	·982	·978	·973	·968	·963	·958	·952	·947	·942	·937	·931	·926	·920	·914	·909	·903	·897	·891	·885	·879
29·3	1·025	1·020	1·015	1·011	1·006	1·001	·996	·991	·986	·981	·976	·971	·966	·961	·956	·950	·945	·940	·935	·929	·923	·918	·912	·906	·900	·894	·888	·882
29·4	1·028	1·024	1·019	1·014	1·009	1·004	·999	·995	·990	·985	·980	·975	·969	·964	·959	·954	·949	·943	·938	·932	·927	·921	·915	·909	·903	·897	·891	·885
29·5	1·032	1·027	1·022	1·018	1·013	1·008	1·003	·998	·993	·988	·983	·978	·973	·968	·962	·957	·952	·947	·941	·936	·930	·924	·919	·913	·907	·901	·895	·889
29·6	1·036	1·031	1·026	1·021	1·016	1·011	1·006	1·001	·996	·992	·986	·981	·976	·971	·966	·960	·955	·950	·944	·939	·933	·927	·922	·916	·910	·904	·898	·892
29·7	1·039	1·034	1·029	1·025	1·019	1·015	1·010	1·005	1·000	·995	·990	·985	·980	·974	·969	·964	·959	·953	·948	·942	·937	·931	·925	·919	·913	·907	·901	·895
29·8	1·043	1·038	1·033	1·028	1·023	1·018	1·013	1·008	1·003	·998	·993	·988	·983	·978	·972	·967	·962	·957	·951	·946	·940	·934	·928	·922	·916	·910	·904	·898
29·9	1·046	1·041	1·036	1·031	1·026	1·022	1·017	1·012	1·007	1·002	·997	·991	·986	·981	·976	·970	·965	·960	·954	·949	·943	·937	·932	·925	·919	·913	·907	·901

30·0	1·050	1·045	1·040	1·035	1·030	1·025	1·020	1·015	1·010	1·005	**1·000**	·995	·990	·985	·979	·974	·968	·963	·958	·952	·946	·941	·935	·929	·923	·917	·910	·904
30·1	1·053	1·048	1·043	1·038	1·033	1·029	1·024	1·019	1·014	1·009	1·003	·998	·993	·988	·983	·977	·972	·966	·961	·955	·950	·944	·938	·932	·926	·920	·913	·907
30·2	1·057	1·052	1·047	1·042	1·037	1·032	1·027	1·022	1·017	1·012	1·007	1·002	·996	·991	·986	·980	·975	·970	·964	·959	·953	·947	·941	·935	·929	·923	·916	·910
30·3	1·060	1·055	1·050	1·045	1·040	1·036	1·030	1·025	1·020	1·015	1·010	1·005	1·000	·995	·989	·984	·978	·973	·968	·962	·956	·950	·945	·938	·932	·926	·919	·913
30·4	1·064	1·059	1·054	1·049	1·044	1·039	1·034	1·029	1·024	1·019	1·014	1·008	1·003	·998	·993	·987	·982	·976	·971	·965	·959	·954	·948	·941	·935	·929	·922	·916
30·5	1·067	1·062	1·057	1·052	1·047	1·042	1·037	1·032	1·027	1·022	1·017	1·012	1·006	1·001	·996	·990	·985	·980	·974	·969	·963	·957	·951	·945	·939	·933	·926	·920
30·6	1·071	1·066	1·061	1·056	1·051	1·046	1·041	1·036	1·031	1·026	1·020	1·015	1·010	1·005	·999	·994	·988	·983	·977	·972	·966	·960	·954	·948	·942	·936	·929	·923
30·7	1·074	1·069	1·064	1·059	1·054	1·049	1·044	1·039	1·034	1·029	1·024	1·018	1·013	1·008	1·003	·997	·992	·986	·981	·975	·969	·963	·957	·951	·945	·939	·932	·926
30·8	1·078	1·073	1·068	1·063	1·058	1·053	1·048	1·043	1·037	1·032	1·027	1·022	1·017	1·011	1·006	1·000	·995	·990	·984	·978	·972	·967	·961	·954	·948	·942	·935	·929
30·9	1·081	1·076	1·071	1·066	1·061	1·056	1·051	1·046	1·041	1·036	1·031	1·025	1·020	1·015	1·009	1·004	·998	·993	·987	·982	·976	·970	·964	·958	·951	·945	·938	·933
31·0	1·085	1·080	1·075	1·070	1·065	1·060	1·055	1·049	1·044	1·039	1·034	1·029	1·023	1·018	1·013	1·007	1·002	·996	·991	·985	·979	·973	·967	·961	·954	·948	·942	·936
31·1	1·088	1·083	1·078	1·073	1·068	1·063	1·058	1·052	1·047	1·042	1·037	1·032	1·026	1·021	1·016	1·010	1·005	·999	·994	·988	·982	·976	·970	·964	·957	·951	·945	·939
31·2	1·091	1·086	1·081	1·076	1·071	1·066	1·061	1·055	1·050	1·045	1·040	1·035	1·029	1·024	1·019	1·013	1·008	1·002	·997	·991	·985	·979	·974	·968	·961	·955	·949	·943

ILLUMINATING POWER OF GAS.

To Correct the illuminating power in Candles DIVIDE by the Tabular number due to Temperature and Pressure.

Gas Office, George Street,
Nottingham, July, 1883.

EXAMPLE:—Observations 1) 8·1
2) 8·
Bar. 29·1 3) 8·1
Ther. 60° 4) 8·5
Tab. No. ·969 5) 8·6
6) 8·6
7) 8·6
8) 8·5
9) 8·6
10) 8·6

Average 8·12
Candles, grs. 40

20) 336·8

Tab. No. ·969) 16·81

Corrected Illuminating Power 17·38

VOLUME OF GAS.

To Correct the volume of Gas as registered by the Station Meter MULTIPLY by the Tabular number due to Temperature and Pressure.

EXAMPLE:—Gas made—2,163,000 cub. ft. at 30·1 in Bar. and 63° Ther.
Multiplying by Tab. No. ·995

2,152,185 cub. ft. corrected volume.

but since there are no losses, as in a great manufacture, and for other reasons to be explained later, the results are always higher than those found in actual working practice. They give, subject to what I shall have afterwards occasion to remark respecting influence of temperature, and containing vessel of distillation, what is in the coal, and what the gas-maker must endeavour to produce on the working scale.

For many reasons, in a large gas undertaking it is desirable to have more practical and extensive information concerning the quality of various coals, and their all-round behaviour in large working plants that experience has evolved. For instance, the retort now-a-days must be of fire-clay, the cost of wear and tear of iron being prohibitory. Then an exhauster is required to relieve the retort of pressure, since a system of counterpoised holders would not be practicable. To enable coal to be tested under the actual conditions obtaining in a modern gasworks, the Gas Committee of Nottingham decided to erect a small gasworks, which is to all intents and purposes part of the manufacturing plant. At one end of one of the retort houses was a coal store, and in this was erected exhausting, condensing, scrubbing, purifying, and gas-measuring plant capable of dealing with one, two, or three sets of retorts in regular work. Thus the expense of any special buildings, retort settings, and gas-holder was obviated, the simple addition being the plant above-mentioned. Thus, gas from sets of retorts as ordinarily worked, could be separately measured and examined, and the fullest information obtained in a practical and useful manner concerning any coal. The hydraulic main connected with the above-mentioned three sets of retorts was by means of diaphragms made into three sections, one for each set, and by suitable arrangement of pipes and valves one, two, or all of these sets could be connected with the special testing plant, or with the plant of the regular gasworks. The gas, then, from one, two, or three sets can be separately carried to a Pélouze and Audouin condenser, then to a steam jet exhauster water condenser, two scrubbers (one for washing the gas with ammoniacal liquor and the other with clean water), to two purifiers charged with lime, small station meter that had been in use for a somewhat suitable purpose, and was of admirable construction and ample size for the purpose, and finally to one of the works' gas-holders. By proper arrangements of steam traps and governors a very perfect vacuum is secured (this is a point of utmost importance), all the connections and plant are of sufficient size, so as to avoid obstructions and to prevent undue and unequal resistances. Thermometers and water gauges are connected to every important part of the apparatus. A small steam pump for circulating the liquor through the first scrubber, water meter to measure the quantity of clean water added, and proper valves to all the plant. The station meter was brought to exact agreement by numerous tests with a standard test meter, made by a celebrated maker, which corresponded accurately with two standard gas-holders made by different competent makers. The whole plant from retorts to meter is perfectly open to observation, and the pipes are above ground. There has been no trace of leakage, but any would be immediately observed. The seals of condensers and scrubbers are connected to a tank accurately made, so as to contain 10 gallons to the inch. In this the tar and liquor made is measured, but as it is sometimes difficult to find the exact level of separation between the tar and liquor, the former at the end of every twenty-four hours is pumped into a separate measuring tank, to confirm the measurements obtained from the other. The coal to the setting of retorts—each of which consist of fourteen single or seven through retorts, 18 × 13, D section, 18 feet through—is weighed on a machine continually checked, but examined also once a year, together with all the others in the works, by the maker. The weighings are also checked by the gross and tare weights of the coal wagons as they come in and go out of the yard. The coke drawn out is also accurately weighed, and its quality and appearance observed. The retorts are charged as regularly as possible to obtain the proper regularity of quality, and generally carry 2 cwt. charges in five hours; but this, of course, is varied if required. In that manner the normal weight of coal per set per twenty-four hours is 6 tons, 14 cwt., 2 quarters. No charge is drawn unless properly burnt off. Each hour the meter index and thermometer and jet photometer and Giroud's verificateur readings are taken and recorded. Every two hours the gas is further tested in a first-class photometer provided with a Methven standard burning town works gas, which is of very regular illuminating power. Daily the gas, on outlet of scrubbers, is quantitatively tested for the slight trace of ammonia which occurs in gas, washed clean to litmus paper; for sulphuretted hydrogen carbonic acid gas and cyanogen compounds. Samples of tar and ammoniacal liquor are taken and tested, a quantity proportionate to the day's make being saved and collectively analysed at the completion of the trial, which generally lasts seven days to secure a good average. Every day also the gas is tested for CS_2. The volume of gas registered is corrected daily by the average of the thermometer readings of the meter and barometer records, by means of the table before referred to. Every effort is made to secure absolute accuracy of observations, and various analyses not above specified are made from time to time, to obtain a knowledge of the coal and its products of distillation, and in contiguity to the two plants is a chemical laboratory, where the analyses are made by approved methods.

With a large testing works like the one at Nottingham, it will be evident that on first starting a trial after the plant has been standing, say for some days, a little time will be required to get it into normal working, since the seals and other parts are charged with tar and liquor from a previous trial of perhaps a totally different coal; the scrubbers, condensers, etc., are drained free from liquid products, and the whole apparatus is filled with stale gas; and even if the plant has been in work immediately before changing over from one coal to another, some of the above conditions will apply. For reasons founded upon the above and similar considerations, the first 24 hours' work, though accurately recorded, is not to be considered satisfactory. We generally arrange for a week's working, which, with the deductions of the first day and Sunday, gives a five days' average. It will be useful to state that subject to the influence of temperature of distillation, as indicated by the volume of gas made per ton, the result "candles per ton," from one day to another is generally satisfactory, and even when an occasional discrepancy arises, it is due often to difficulties in photometer testing, and would be removed if, instead of calculating the results from the bar photometer, we employed the average of the hourly readings of the inferential tests, such as Giroud's verificateur or Lowe's jet photometer. The yields of coke and tar from day to day are fairly concordant, but the variations of ammonia are always large and quite outside any possibility of experimental error. This matter of ammonia has received a deal of attention, and the results must be accepted as an index of the true behaviour of this nitrogen product of coal distillation. We find in testing the crude gas of our gas-works the same variations from hour to hour, and I have frequently had occasion to observe this peculiarity in London. I have been making some inquiries into the cause of this variability, but up to the present am only in the position to say that it is not due to certain influences I had suspected.

From a proximate analysis alone we can gather but little information as to the value of a coal for gas-making purposes; it might expose a bad coal, but could hardly make as sure of a good one. The coke result accords, in my experience, remarkably well with the more practical ones. Thus, a Derbyshire coal gave 38 per cent. v.m., leaving 62 per cent. of coke, or 12·4 cwts. per ton. The laboratory practical analysis with iron retort (9 experiments) 12·37 cwts. per ton, with clay retorts (27 experiments) 12·39 cwts. per ton. In another case—

Day of trial.	Cubic feet per ton corrected.	Illuminating power.	Candles per ton of coals.	Coke cwts. per ton).	Tar (gallons per ton).	Ammonia (gallons per ton of 10oz.).	Sulphur other than H_2S, grains p. 100 c. ft.	Grains of CO_2 p. 100 c. ft. of washed crude gas.	Grains of H_2S p. 100 c. ft. of washed crude gas.	Cyanogen in grains of NH_3 p. 100 c. ft.	Average of jet photometer.	Average reading of Giroud's verificateur.	Carbonic oxide per cent.
CANNEL COAL													
2.	9,515	23·17	44,231	12·70	21·10	28·53	—	3017	610	39·38	4·12	89·5	6·3
3.	10,147	22·09	44,829	11·39	20·49	31·27	21·53	3031	539	38·18	4·50	92·3	7·2
4.	10,151	22·33	45,334	12·72	20·56	27·03	25·06	2917	570	36·61	4·20	90·5	8·3
5.	9,852	22·69	44,708	11·98	19·95	23·42	26·16	3142	481	38·00	4·15	—	8·0
6.	10,000	22·56	45,120	11·91	19·95	26·85	21·17	3391	363	37·67	4·17	95·0	7·6
NUTS FROM A DERBYSHIRE COLLIERY.													
2.	9,705	16·96	32,919	12·39	11·55	20·55	52·96	1227	935	—	6·05	122·5	5·9
3.	9,818	16·51	32,518	12·35	12·44	20·92	58·86	1111	1010	38·56	6·30	124·6	6·4
4.	9,558	16·39	31,331	12·61	11·55	19·01	55·36	1332	986	38·26	6·50	120·3	6·4
5.	9,818	16·31	32,124	11·71	11·36	17·45	56·81	1250	990	36·53	6·00	124·3	—
6.	9,852	16·01	31,516	12·50	11·55	20·75	—	1189	985	37·55	6·06	125·2	6·2
CANNEL COAL.*													
5.	10,298	22·26	45,817	12·41	22·72	25·30	23·17	2818	231	36·92	4·22	91·16	6·8
6.	10,111	23·07	46,652	12·20	19·13	28·66	22·05	—	—	—	4·26	95·25	6·3
7.	10,161	22·16	46,363	12·57	21·10	21·82	35·02	—	—	—	4·27	95·66	6·9
8.	9,996	22·18	44,342	11·83	21·00	22·70	27·86	2600	337	—	4·20	94·12	7·0
9.	9,918	23·10	45,960	11·43	23·80	28·12	35·81	2912	363	38·78	4·16	91·00	6·7

* This cannel is from an entirely different seam to that which produces the cannel that heads this list.

It will be expected that I should say something about the practical values of the various methods of analysis.

As to elementary analysis, can one, from an inspection of numbers giving the elementary composition of a coal, offer a positive or even approximately valuable opinion concerning its uses to a gas-maker? I would reply certainly not, as far as our present information goes. It would take too much time to enter into my reasons for this statement, but an inspection of published analyses and gas-making results of various coals will, I think, convince anyone of the justness of the above-expressed opinion. I think, however, that, where possible, an elementary analysis should be made of samples under practical test, for the composition of the same seam varies so, that unless the two analyses represent the same bulk, anything like a close comparison could not be made.

It is equally impossible also to form a definite or valuable judgment of the gas-making properties of a coal from the proximate analysis. Of course, broadly, we can assume that the gas-making value varies with the volatile matter, less moisture—for instance, Australian shale, with 77 per cent. v.m., gives 140,000 candles per ton, and a Derbyshire or Yorkshire coal, with 35 per cent. v.m., about 35,000 candles per ton.

For absolute practical information respecting a gas-making coal, we are driven to a practical analysis.

As to the value of proximate analysis—when other practical methods are used, the results peculiar to proximate analysis—viz., volatile matter (not including water at 212°) and coke are not so valuable, except perhaps in confirming what is found in practice.

Proximate analysis...... 63·83 per cent., or 12·76 cwts. per ton.
Laboratory practical analysis .... 12·77 „ „
Works practical analysis .... 12·69 „ „

The laboratory practical analysis with iron retort is of great value, but it will be necessary in interpreting the results to recollect that the method of heating the coal is different to that used in practice with clay retorts. In the former, the heat is quickly conveyed through the metal vessel, and there is no doubt in my mind that an iron vessel exerts a chemical action upon the gas made therein. As far as volume and quality of gas are concerned, we always obtain better results with the small iron retort than with clay.

But it is most important to consider the influence of temperature upon the results. It is customary to consider that within certain practical ranges of temperature the product of volume of gas yielded by its luminosity—*i.e.*, cubic feet per ton × illuminating power per cubic foot per hour—is a constant for each coal. As we decrease the one factor, we proportionately increase the other. When we say that a ton of coal contains—say 9000 cubic feet per ton, we do not mean that there is that fixed quantity and no more nor less in it, but simply that under certain conditions of distillation that quantity of coal yields 9000 cubic feet, and it is implied that by either raising or lowering the mean temperature of distillation, more or less gas of more or less luminosity is produced. Now, it is important to enquire whether the variations in the two factors are proportional to each other, or, in other words, whether the product of quantity by luminosity is a constant for each coal. It is difficult to know what is the current opinion on this point. Dr. Schilling in his "Handbook of Coal-Gas Lighting," speaks of the economy of "high heats" in a general manner, but also publishes the following table, from which it might be inferred that with an increase in the "heat" there is a decrease in the total luminosity afforded by a given weight of coal.

	An Increased yield of gas.	A decreased illuminating power.
Zwickau Coal......	18 per cent.	14 per cent.
Saarbrück Coal ...	18 ,,	21 ,,
Bohemian Coal ...	25 ,,	30 ,,
Old Pelton Coal ...	24 ,,	38 ,,
Westphalian Coal	15 ,,	40 ,,
Stockheim Coal ...	20 ,,	42 ,,

With the exception of the first result on the list, my own experience differs from his, for I have always found, with carefully and properly conducted experiments, that with any coal I have experimented with, the product of volume by illuminating power increases with the volume.

In a paper on the influence of temperature of distillation in the composition of coal gas, to be found it the *Journal of the Chemical Society*, March, 1884, I showed the influence of temperature in bringing out the light-affording power of coal, thus :—

Gas coal distilled at a temperature to yield in cubic feet per ton.	*Illuminating power per 5 cubic feet per hour.*	*Candles per ton, or yield by candles per cubic foot.*
8,250	20·5 candles.	33,825
9,693	17·8 ,,	34,510
10,821	16·75 ,,	36,250
12,006	15·6 ,,	37,460

Taking the first and last results, we find that with an increased yield of 45½ per cent., there is only a decrease in the illuminating power of 24 per cent.; and that there is a net gain of 11 per cent. by working at the higher temperature. The following table will indicate this for a number of different coals. It is important that I should emphasise this fact that the product of volume by illuminating power increases with the volume.

Gas per ton. Cubic Feet.	Illuminating Power, Candles.	Candles per ton (Clay retorts).
	CANNEL COAL.	
9350	24·99	46,731
9536	25·08	47,832
9798	25·27	49,519
9801	25·79	50,553
	YORKSHIRE GAS COAL.	
10,735	15·26	32,763
10,882	15·49	33,712
11,212	15·24	34,174
	SHALE.	
7451	25·48	37,970
8160	24·16	39,429
8311	23·78	39,527
	CANNEL.	
9,411	20·23	38,077
10 909	19·15	41,781
	DERBYSHIRE COAL.	
9,411	15·34	28,873
10,147	14·94	30,319
10,294	15·30	31,499
10,313	15·35	31,661
10,757	15·27	32,852
	DERBYSHIRE NUTS.	
9,558	15·49	29,611.
10,454	15·30	31,989

I have before compared the yields of coke obtained by proximate analysis and practical methods. I now propose to consider the results obtained by the two practical methods in ordinary use - viz., the small iron retort and setting of clay retorts identical to those used in regular gas manufacture. The iron retort is one inch, the clay retorts are three inches thick. For the same temperature difference between the inner and outer surfaces of these vessels, there is in the same time 170 times more heat passed through the iron than through the clay.' The flue or outside temperature employed in the clay settings if applied to iron retorts, would bring them like water running into the fires in a few hours. Yet in working iron retorts in perfect safety, it is possible to get an efficiency of 140 cubic feet per square foot of heating surface out of them, a result much above the average of clay retorts. The apparent temperature applied to the coal is less with iron than with clay. Yet the effective temperature in distilling coal can be at least as great as is customary with the latter. Any determination of the practical or effective temperature of retorts by pyrometer at the end of a charge, or by the colour or appearance of the retort, is likely to prove illusory, for it is easy to get up a good appearance in them by undercharging. The best test to my mind is the amount of work they will perform in distilling coal expressed by the quantity of gas produced per superficial foot of heating surface per 24 hours.

I have found that with the small iron retort a better result is always obtained than with the large experimental works, but the difference varies with the kind of coal. With the very finest coking coals the difference is very small, and as the coking quality of the coal decreases, so the difference between the two methods of testing increases. With coals (not cannels) which scarcely intumesce at all the difference becomes very high.

Cannels also vary in the same manner, the difference in the results being always connected with differences in the qualities of the residues of distillation or cokes. This point is illustrated in the following table :—

	CLAY RETORT.		IRON RETORT.		Difference per cent.
	Cubic ft. per ton.	Candles per ton.	Cubic ft. per ton.	Candles der ton.	
Average of 12 Derbyshire and Yorkshire Coking Coals........	10,110	33,600	11,010	36,600	9
Average of 5 Derbyshire and Nottinghamshire Non-Coking Coals............	9,156	27,330	10,430	33,820	23
Average of 11 Cannels from various Districts	9,930	42,360	10,630	49,850	18

I have stated the volume of gas produced per ton, so that in regarding the above figures, allowance can be made for difference naturally due to temperature, as measured by yield of volume.

Since the gas from the iron retort was not scrubbed, a deduction of about 3 per cent. should be made from the results of the small apparatus, to compensate for the slight loss of illuminating power suffered by the gas of the large experimental works in the washing process. When this allowance for washing is made there is really little difference between the two methods of testing in the case of caking coals, but the difference is quite irreconcilable with some coals. There are two cannels of a shaley character included in the above average of 14 cannels, which give differences of 22 per cent. and 31 per cent. respectively; and it is remarkable that these two materials yield residues on distillation hopelessly valueless as fuel, being little else than an earthy ash. The one which gives a difference of 31 per cent. was introduced to me with a splendid analysis made probably in an iron retort. On submitting the coal to test, it was found to be of very little value when carbonised in clay retorts, though it tested fairly well in the small iron vessel. It is known that the weight of the fixed residue of distillation in proximate analysis is dependent on the manner, degree, or extent of heating, and I have shown above that the amount of valuable gaseous products obtained from a coal will vary with the manner and extent of heating; and further, that the nature of the base or fixed residue of distillation has an important influence on the behaviour of coal submitted either to rapid or to slower distillation. It is within my experience as a gas-maker that with the same coal the character and appearance of the coke produced in gas manufacture is largely affected by the method employed in carbonising it. I have in my mind a class of coal the smudge from which produces the finest furnace coke (when coked in beehive ovens) in the world, and which is regarded by gas-makers as a splendid gas coke producing coal. This coal, if carbonised in clay retorts, working off 2cwt. charges in 5 hours, and affording about 115 cubic feet of gas per 24 hours per square foot of heating surface, gives a fine large metallic looking coke of hard steely appearance. If the retorts are worked at a higher average temperature, so as to burn off the 2cwt. charge in 4 hours, and to produce about 140 cubic feet of gas per square foot of heating surface, the coal will give a coke of inferior appearance to the other, being smaller and less metallic in appearance, with many black pieces. If carbonised in iron retorts carbonising 1¾cwts. in 5 hours, and giving 140 cubic feet of gas per square foot of heating surface, the coke is small, black, and of appearance much inferior to the others.

Enough has been said to show that the results given by the small iron retort in common use for testing gas-coals cannot be accepted as authoritative in these days, when only clay retorts are used in large works; and it must be in the experience of all gas manufacturers that many coals do not justify in practice the values assigned to them by analysts, who use what is called the laboratory analysis. It will be interesting also to know how far the results obtained in our gas works agree with the values assigned to the various coals we use by the experimental works in which they are all tested. I have carefully compared for a long period the actual working results of our daily manufacture with the theoretical average value of the mixture of coals and cannel we employ, calculated from the values of the respective coals as determined in the experimental works in their various proportions, and I find that we exceed in practice the theoretical field so calculated by 3 per cent. This amount is not large, but it is constant, ranging from about 2 to 4 per cent.; and is further, I believe, beyond the range of experimental error. This slight improvement is no doubt due to the admixture of low and high power gases, obtained from a 70 per cent. of coking coal, and 30 per cent. cannel coal before condensation commences, with the result that the low power gas carries forward some of the hydrocarbons of the cannel gas that would otherwise be condensed and removed from the gas by the tar. I may say that our station meters have been compared and standardised by the same standard meter used for standardising the experimental works station meter, and that the illuminating power used in calculating the candles per ton we obtain in our manufacture is the average of our official tests made in the centre of the town.

It is unfortunate that we have no proper classification of all the various kinds of coal which nature has produced. Such terms as caking splint coal, cherry coal, peacock coal, cannel coal, slate coal, leaf coal, coarse bitumenous, semibitumenous, are not sufficiently explicit. The Germans, I think, have a better, but even theirs I find wanting, as it is customary to classify a non-homogeneous coal, consisting of many varieties, under one name that may express its few united qualities more or less perfectly, but not satisfactorily. A piece of coal I have selected for your inspection contains at least three widely different kinds, first you will see layers of vegetable charcoal, then a material very like pitch, and finally cannel coal. The part forest fires may have played in the formation of these qualities has not received attention. It is difficult to examine this coal without imagining that it is the final result of charred wood fibre. The pitch having been a distillation product, and the cannel coal which occurs in a basin or huge pond, being an earthy base permeated with tarry distillation matters, and deposited in a liquid form. It is very common to find these three sorts of coal in contiguous layers, and yet the coal may be classed as caking coal. The Germans, I think, have given the names Russkohle and Pechkohle to the two first-mentioned varieties, but it is difficult, of course, to know always what German writers understand by the numerous titles they have for different sorts of coal. A complete classification and examination of our various British coals is wanting, and we have, I am sorry to say, no work similar to the extensive work of Drs. Geinitz, Fleck, and Hartig, on the coal of Germany. I fear we are accustomed to leave too much to individual effort.

Estimation of Sulphuretted Hydrogen and Carbonic Acid in Crude Coal-Gas.—For many reasons I prefer, for the estimation of hydrogen sulphide and carbonic anhydride in impure coal-gas, a method founded upon the increase in weight of absorption tubes; a method which will admit of the use of a tolerably large quantity of gas collected regularly during an interval of time sufficiently long to afford an idea of the average composition of the gas supply to be tested.

I have adopted as the reagent for absorption of hydrogen sulphide in absorption-tubes, a cupric phosphate, which is an impure di-, tri-, ortho-phosphate.

The cupric phosphate for use in absorption-tubes is prepared somewhat in the following manner:—

Solutions of—

2lb. hydrogen disodium phosphate in 1 gallon of water, and

2¼lb. cupric sulphate crystals in 1¼ gallons of water, are mixed with vigorous stirring, and the resulting bright blue precipitate washed by decantation and then dried in a water-bath to about 100°. The material sometimes is light and powdery, sometimes hard, and then requires crushing in a mortar. Its action with hydrogen sulphide is very sharp indeed.

The **U**-tubes used in these experiments are preferably of the new form with hollow glass round stoppers, serving as stopcocks, the inlet and outlet connections being small pieces of glass tubing fused into the sides or the tubes. A small hole in the stopper corresponding to the hole in the side of the tube, where the small glass tubing is fused on, makes the connection. A small turn of the stoppers closes or opens the tube. The tubes are weighed charged with clean coal-gas, which on the same gas supply, varies but little in specific gravity.

The stoppers are lubricated with resin cerate, and with this exception are perfectly clean. A small plug of cotton-wool in each stopper prevents the mechanical carriage of fine proportions of the reagents by issuing gases. Under these circumstances, I find that closed tubes charged with coal-gas suffer no appreciable variations in weight during a period of two days. These tubes are very useful indeed, and are far superior to the old form with corks.

I find that absorption-tubes charged with cupric phosphate gain in weight under the action of clean coal-gas; but the increase of weight soon reaches a limit, and the phosphate becomes "saturated," and to attain this saturation 3 cubic feet of clean dry coal-gas should be slowly passed through the tubes.

The phosphate tubes are charged in the following manner. The tubes are cleaned and dried, great attention being paid to the ground portions of the tubes and stoppers. A plug of cotton-wool is placed in the bend of the **U**-tube. Then one leg of the tube is charged with the cupric phosphate in a suitable state of subdivision, care being taken that the rough and fine portions of the powder are regularly distributed, for if all the rough portions are on one side and the fine on the other, a passage of the gas down the side where the coarse portions are takes place, and the finer portions never come into action. The other leg is charged with powdered calcium chloride. The ground portions of the tubes are wiped free from dust, and the stoppers, fitted with plugs of dry cotton-wool in the hollow places, are lubricated with a little resin cerate, and put in position. A current of dry gas-coal to the extent of 3 cubic feet is passed through, and the tubes are ready for weighing. A 6-inch **U**-tube charged in this manner will be capable of absorbing 20 grains of hydrogen sulphide, and if in the case of gas containing 10 grains of hydrogen sulphide per cubic foot, the experiments be made upon quantities of half a cubic foot, each will therefore serve for four analyses without being recharged. The calcium chloride will serve much longer.

The soda-lime tube for carbonic anhydride absorption is charged in the same manner, one half full of soda-lime, and one half of calcium chloride. I have found it necessary to use the soda-lime in a moist condition, for, when quite dry, soda-lime has a much feebler absorptive power for carbonic anhydride.

The requisite degree of moistness is attained by exposing it to a moist atmosphere for 12 to 18 hours. There are no inconveniences attending soda-lime in this condition. These soda-lime tubes remain very constant in weight when clean pure coal-gas is passed through them, and therefore do not require saturating in the same manner as the phosphate tubes: a small quantity of dry pure coal-gas should, however, be passed through to expel air, previous to their being weighed.

A 6-inch tube, charged with soda-lime in the manner above described, serves for three analyses of half a cubic foot each on gas containing 10 to 12 grains carbonic anhydride per cubic foot.

In cases where ammonia also exists in the coal-gas to be analysed for hydrogen sulphide and carbonic anhydride, it will be necessary to effect its removal before the coal-gas reaches the weighing tubes. That calcium chloride effectually removes ammonia from coal-gas I have frequently had occasion to notice, and Dibbits (*Zeits. für Analyt. Chem.*, **15**, 1876, 124) has made the same remark. But to maintain the neutrality of the calcium chloride I remove the ammonia from the coal-gas before drying, and for this purpose pass the gas through a 12-inch **U**-tube filled with broken pumice saturated with syrupy phosphoric acid.

In the drawing off of samples of coal-gas for analysis, certain precautions are indispensable, since vulcanised and iron tubing when new remove hydrogen sulphide from crude coal-gas, so that it is necessary to have the services conducting the coal-gas from the source to be tested to the absorption-tubes as short as possible, and saturated by previous use with crude coal-gas. It is further of importance to keep a tolerably quick current of gas through the iron pipe, in order to prevent the gas from getting "stale."

The iron service should be fitted with a four-way piece and three cocks. One serves to take the gas to the train of apparatus; another to be left open during the analysis, blowing gas away, and thus maintaining a rapid current in the iron pipe; the third cock can be used for other purposes, such as examination of the gas for ammonia, &c. Before making an analysis it is necessary to blow a little gas away in order to clear stale gas out of the iron service pipe, and to bring a fresh supply along.

The complete apparatus for the estimation of hydrogen sulphide in coal-gas can now be described.

The first piece in the train is the syrupy phosphoric acid tube, which can however be dispensed with in cases where the gas has been thoroughly washed free from ammonia. This tube is directly connected with the cock on the iron pipe by means of a small piece of "saturated" vulcanised tubing. The outlet of this tube is connected to a large drying cylinder filled with small pieces of calcium chloride free from alkalinity. The outlet of this drying cylinder is a **T**-piece, which carries on one arm a small piece of vulcanised tubing and screw-clamp for the purpose of blowing a little gas away before and during each experiment. The other arm of the **T**-piece is connected to the inlet of the cupric phosphate tube, which in its turn is connected with the soda-lime tube, and a gas meter connected to the outlet of the soda-lime tube completes the apparatus. The meter most suitable for the purpose is the test-meter, as used by the gas referees for the "sulphur" test. It it advisable to bring the pointer up to within a division or so of the zero by means of the key, and then to blow through until the pointer exactly covers the mark. Any error due to slackness in the gearing of index is thus avoided. Before starting an experiment, gas should be blown away at the outlet of the drying cylinder, as well as at the cock on the iron service.

When the tubes have been weighed full of clean coal-gas, and all the pieces of the train properly joined together, the stopcocks are turned on, and the gas allowed to traverse the apparatus at a moderate rate, which is often more dependent on the pressure of the gas than the will of the operator. From a quarter to one half a cubic foot per hour is a convenient rate, and one well within the limits at which the complete action of the reagents is obtained.

The quantity of gas used for each experiment is a matter of choice, and will be determined by the quantity of impurity in the coal-gas under analysis. When the required quantity of gas has passed through the tubes, the inlet stopper of the phosphate tube and the outlet stopper of the soda-lime tube are turned off, and the two tubes are taken off together, and connected in their proper order to a supply of desiccated clean gas, in order to drive the gas in the phosphate tube through the soda-lime tube. The stopper can then be turned off and the tubes weighed. Of course, before weighing these tubes, they must be wiped perfectly clean and dry; but as this operation causes them to be a little light, they should be left for about five or ten minutes in the balance case before being weighed. The results corrected for temperature, &c., are best calculated to a cubic foot of clean coal-gas at 60° F. and 30in. barometer.

Since washed but otherwise unpurified coal-gas contains about ·6grain per cubic foot of cyanogen, the results obtained by this method are slightly high. The absorption of cyanogen in the cupric sulphide is not complete, about one half going forward into the soda-lime tube. The error falls equally on each tube.

Cyanogen.—This body is estimated by passing a measured quantity of gas free from ammonia, but otherwise unpurified, through a **U**-tube filled with soda-lime, and then making a combustion of the residue as in an ordinary nitrogen determination.

INTERNATIONAL INVENTIONS EXHIBITION, LONDON.

ADDENDUM TO REPORT ON THE EXHIBITS RELATING TO THE CHEMICAL INDUSTRIES.

BY WATSON SMITH,

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Beeswax.—Specimens were shown of the wax, air-bleached and bleached by the potassium bichromate method. The latter process, whilst giving the whiter product, yields a crystalline wax less suitable for burning than for the use of druggists.

Samples of church candles and carriage lights made from the wax were shown, and it is stated that Messrs. J. C. & J. Field made wax candles in 1680.

Spermaceti.—This is purified from the "head-matter" and crude oil by "bagging," or a process of filtration in which the weight of the substance is made available. About ten per cent. is recovered. Melting-point, 43° to 44° C.

This material is used for high-class candles, popular on account of the absence of the tendency to bend when placed in a warm atmosphere. Specimens of standard sperm gas-candles were shown, burning 120 grains per hour.

I am informed that Messrs. J. C. & J. Field made the first sperm candles in 1760, and that over 220 tons of wax and spermaceti are now manufactured per annum by the firm, about 400 tons of wax and sperm candles being employed in England and Ireland alone.

Stearin.—Specimens were shown prepared both by the "autoclave" process and by lime saponification. The product of the latter process shows the highest melting-point—viz., 56° C., and is better adapted for working up inferior greases.

The original candles from Milly et Cie., "*Bougies de l'Etoile,*" imported by J. C. & J. Field in 1835, were exhibited.

Paraffin.—The cruder material is refined by treatment with naphtha, and by the "banding" process—*i.e.*, passing through cogged wheels in an alkaline solution, which emulsifies the coarser oils. Melting-point of the refined product is about 51° C. J. C. & J. Field state that they were the first to make paraffin candles, and that they originally contracted with the Irish Peat Company and Young's Paraffin Company for the sole supply of their produce.

Ozokerite.—Introduced by F. Field, F.R.S., in 1872. The crude product from Gallicia is treated with superheated steam, when a purer paraffin distils over. The distillates obtained are—

(i.) A gaseous hydrocarbon, to the extent of about five per cent.
(ii.) A volatile naphtha. (About three per cent.).
(iii.) A "vaseline" product called "Ozokerine." (About six per cent.).
(iv.) A soft paraffin. (About twelve per cent. Melting-point 44½° to 46° C.).
(v.) A white paraffin (*Ozokerite*), with a melting-point of 61° C.
(vi.) A black waxy residual melting at 77° C.

The gas and naphtha are said to be useless. The ozokerine has a sale amongst chemists and hairdressers and in the hospitals, as a basis for pomades and unguents. It is also imported to France (Grasse) as a substitute for lard in "*enfleurage.*" The *soft paraffin* is employed in the manufacture of cheap candles, retailing at ½d. and 1d. This employment gave rise to the use of paraffin scale for that purpose, which has resulted in the death-blow to the tallow and composite candle trade.

The White Paraffin, Ozokerite proper, is employed for making the well-known ozokerite candles, which, it is stated, melt not only about six degrees (Centigrade) higher than any other paraffin or stearin candle, but bend at a much higher temperature. The flame of the candles is small and very brilliant. It is stated that equal candles of ozokerite and sperm give lights differing in the ratio of 10 : 7.

The Black Paraffin is largely used by electrical engineers as an insulator. Matthiessen found that it gave more complete insulation and possessed less induction capacity than any other known substance.

F. Field has patented an elastic compound of this wax with indiarubber, which has been adopted to some extent in the manufacture of cables.

Ornamental Candles.—Of these a large number is exhibited. Messrs. Field state that they patented every description of ornamental candle now made. The cable candles they still make with the lathe, having abandoned the machine, as giving results good, it is true, as regards quantity, but not equal as regards quality to the lathe candles. It is stated that by the use of the lathe patented by Messrs. Field, eighteen varieties and sizes can be turned out, ornamented with vertical and spiral stripes and six different cable patterns. To do this otherwise would require a corresponding number of machines, and certain varieties of the ornamental candles referred to cannot be made by any other than the lathe method.

Self-fitting Ends.—Of these Messrs. Field state that they were the original patentees, and for fourteen years have been the sole manufacturers. They still retain the original form—namely, with broad obtuse angled flutes, stating that ends shaped thus, by spreading out, perpetually resist the pressure, being fixed in their receptacles after the manner of wedges. The firm has, moreover, patented a very simple "wedge-end," which has the advantage of being made all in one piece like an ordinary candle.

Aërated Candles.—The patent for these was taken up in 1870 from M. Urbain. At the Exhibition the candles were made in a French machine, which for simplicity and efficiency appears superior to the English specimens. The rods which produce the perforations in the bodies of the candles are so constructed as not to crack the sides when withdrawn.

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[*] Omitted on page 563, through information arriving too late for press.

Candle Machinery.—Besides the machine last referred to, Messrs. Field exhibit a new patent machine for making the self-fitting ends. In the old method the caps that moulded the conical butts are withdrawn simply by hand, and replaced in the same manner, entailing a great deal of labour and sacrificing a considerable quantity of wick. In Field's machine a lever separates the coned moulds, thus allowing the candle to be wound out in the ordinary way. The time taken in liberating one set of candles and filling another is five minutes, in the old machine from twenty to thirty.

Soaps.—From the oleic acid obtained from the autoclave and saponification processes, J. C. & J. Field manufacture a brand of soap known as the "Pure Oil Soap," much used by dyers, etc. Their speciality in toilet soaps is the newly-patented "Samphire Soap," which is saponified by the use of iodised potash obtained from the ashes of seaweed, and palm and olein oils. The resulting soap is subsequently milled, after completely expelling every trace of water, and is de-alkalised by the introduction of salicylate of ammonium.

Journal and Patent Literature.

I.—GENERAL PLANT, APPARATUS, AND MACHINERY.

An Improved Substance for Lining or Covering Steam Boilers, Pipes, and other objects. H. J. Allison, London. From Messrs. Sophus, Dohlmann & Company, Copenhagen, Denmark. Eng. Pat. 4343, April 8, 1885.

SAWDUST, chopped straw, cork, bark or other similar light substances are mixed with soluble glass and chalk or lime, in a pulverised state. The mixture thus obtained—formed into pieces adapted to the article it is intended to cover—is thoroughly impregnated with a boiling solution of calcium, magnesium or barium chloride or sulphate of alumina, and after washing in boiling water to free it from soluble salts, the substance, which is now incombustible, is ready for use.—A. W.

Improvements in Filters. E. M. Knight, Liverpool. Eng. Pat. 12,287, September 11, 1884.

A CYLINDRICAL vessel is formed with a concentric perforated cone; this is covered with asbestos cloth, and the annular space filled with charcoal, held down by a perforated cover, through which the water to be purified is admitted to the charcoal filtering bed. The whole is suspended in a porcelain vessel of the usual shape.—C. C. H.

Improvements in Filters. P. A. Maignen, London. Eng. Pat. 13,345, October 8, 1884.

THE conical or V-shaped filtering frames, such as hitherto used by the patentee, are made so that the lower part forms a wedge joint with the side of the vessel in which they are placed.—C. C. H.

Improvements in Filters. C. E. Gittins, London. Eng. Pat. 14,019, October 22, 1884.

THE filter consists of a series of chambers formed from perforated plates and distance frames. These are arranged alternately as supply and discharge chambers, communicating with closed inlet and outlet conduits, as in a filter press. The plates are covered with asbestos or other cloth, and the interior of the chambers filled with charcoal. The whole apparatus is provided with a nozzle, so as to be attached to the tap of a water supply.

—C. C. H.

II.—FUEL, GAS, AND LIGHT.

The Influence of Water on the Combustion of Carbon Monoxide, and the Occurrence of Hydrogen Peroxide in this Combustion. M. Traube. Ber. 18, 1890.

DIXON has shown (*B. A. Rep.* 1880, 503; *Chem. News*, 46, 151; *Phil Trans.* 1884, 617) that a perfectly dry mixture of carbon monoxide and oxygen cannot be caused to explode either by incandescent platinum wires, or the continued passage of sparks from a Ruhmkorff's coil. The author has found, in confirmation of this result, that carbon monoxide already burning, is extinguished in a dry atmosphere. The author considers the influence of the water to be represented by the following equations:

$$(1)\ CO + 2OHH + O_2 = CO(OH)_2 + H_2O_2$$
$$(2)\ H_2O_2 + CO = CO(OH)_2$$
$$(3)\ 2CO(OH)_2 = 2CO_2 + 2H_2O.$$

That hydrogen peroxide is produced in the combustion of carbon monoxide, is shown by bringing the flame of the gas into contact with water, and then showing the presence of the peroxide in the water by the usual tests.

—F. L. T.

The Production of Hydrogen Peroxide in the Combustion of Hydrogen. M. Traube. Ber. 18, 1894.

THE author failed to prevent the explosion of mixtures of oxygen (or air) and hydrogen by the electric spark, by having the mixture as dry as he could possibly make it, thus confirming Dixon's results. He confirms Schuller (*Wiedem. Ann. Chem. Pharm.* (1882) 15, 289) that hydrogen peroxide is produced by burning hydrogen in air, and finds by allowing the burning jet to impinge on the surface of water, that 100cc. of water can take up as much as 290mgrm. of the peroxide of hydrogen, figures indicating what seems to be the maximum concentration obtainable. The greatest amount of hydrogen peroxide produced from the combustion of 1 litre of hydrogen, was 0·0113grm. The size and shape of the jet at which the gas was burnt, the pressure, and the amount of air or oxygen, if any, mixed with the gas before burning, all exerted a perceptible influence on the amount of hydrogen peroxide produced.—F. L. T.

Influence of some Metals on a Mixture of Acetylene and Air. F. Bellamy. Compt. Rend. 100, 1460—1461.

THE gases are mixed in a glass apparatus resembling a Bunsen's burner. If a platinum or silver wire, scarcely red-hot, be held in the mixture, it causes a detonation, but becomes only seldom, and then transiently and partially, white-hot; on the other hand a red glowing copper wire becomes, in a few seconds, white-hot, and then generally produces an explosion. Iron gives rise to the same phenomena, but with greater difficulty.

—F. L. T.

Combustion in Dried Gases. H. B. Baker. J. Chem. Soc. 1885, 349.

AS H. B. Dixon has shown (*Chem. News*, 46, 151; and *Phil. Trans.* 1884, 617) that a dry mixture of oxygen and carbon monoxide cannot be caused to explode by the electric spark, the author has investigated whether moisture is necessary in the combustion of the elements. Amorphous phosphorus, that had been washed with water and dried in a current of carbon dioxide at 100°, and afterwards in a vacuum at 150°, or wood charcoal that had been heated to bright redness in a current of chlorine, were sealed up with oxygen in difficultly-fusible glass tubes, some of which were slightly bent, and held phosphorus pentoxide in one limb. After about a week the phosphorus and charcoal were heated, but no ignition took place, whilst the same substances placed in similar tubes with moist oxygen, burnt as usual. Similar experiments with charcoal, in which the tubes were raised to redness, showed that, in the dry tubes, about half the oxygen remained uncombined; whereas, in the moist tubes, no trace of free oxygen was present. In the dry tubes visible combustion never occurred. These experiments, and some others where the charcoal